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ORIGINAL COMMUNICATIONS AND TRANSLATIONS.

A REVIEW OF DR. OTIS FREDERICK MANSON'S ESSAY ON MALARIAL PNEUMONIA,

By W. T. Howard, M.D., of Warrenton, N. C..

Presented to the State Medical Society of N. C., May, 1858.

What is "malarial pneumonia"? Is it a disease so distinct and different from ordinary pneumonia as to be "unlike in character and nature"? Or is it almost always but the ordinary form, complicated by the impression of a malarial law? By W. T. Howard, M.D., of Warrenton, N. C., presented to the State Medical Society of North Carolina, May, 1858.

In the transactions of the Eighth Annual Meeting of the Medical Society of the State of North Carolina, 1857, there appeared a paper on "malarial pneumonia" by Otis Frederick Manson, M.D., which struck us as not a little extraordinary. If we apprehend correctly the language of the introductory remarks,* it states expressly or by necessary implication :

* In order that the reader may judge for himself, and that we may do Dr. Manson no possible injustice, we insert the passages to which we particularly allude, and to which we ask especial attention :

"It cannot be doubted but that *great confusion* exists at present in regard to the various forms of pneumonia. It perhaps has occurred to every experienced physician, to have seen them all included in the same category, and treated in the same manner; errors to be deeply deplored, attended as they often are by the most disastrous consequences. Inflammation of the lungs, like inflammation of any other structure, may be altogether different in its nature, complications and terminations; and though technically included under the same term, its various forms require as totally opposite modes of management as the most dissimilar disease of organs the most remote in situation and

1. That there exists among the medical men of the South, great confusion of ideas, and consequently great ignorance and want of skill in regard to the diagnosis and treatment of the several forms of pneumonia.

2. That the medical men of the South are greatly to be blamed for this deplorable state of things; particularly and specifically inasmuch as that, "studying the works almost solely of British and Continental writers, which, however perfect in all points for those regions, are sadly deficient for us, *we force the forms of our diseases to bend to their descriptions, and blindly follow a treatment intended for varieties of disease we never witnessed*"; that, therefore, in this matter, we are guilty of "*consummate and fatal folly;*" nay more, and further, that this condition of things is permitted and perpetuated by the present active and experienced members of the profession, "in inglorious ease or censurable indifference," although it is known that the coming generations of physicians will encounter the same trials and difficulties, and suffer the same disasters as their predecessors; and (as if in proof and illustration of these charges)—

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unlike in function. One practitioner, with an eye solely fixed on the lung affection, resorts to the active antiphlogistics of sanguineous depletion and antimony, whilst another, warned by the results of such means, adopts the expectant, or stimulant plan; whilst a third, led on by that spirit of generalization which has been the bane of our science, appeals to Cinchona in all cases, and under all circumstances.

"*Who is to blame?* Assuredly *we* of the *South!* Studying the works almost solely of British and Continental writers, which, however perfect in all points for those regions, are *sadly deficient for us*, we *force* the forms of our diseases to *bend* to their descriptions, and *blindly follow a treatment intended for varieties of disease we never witnessed. Consummate and fatal folly!* Yet, year passes year only to note its repetition, whilst we, reclining in inglorious ease or censurable indifference, view the perpetuation of the error, knowing too that hundreds are yearly added to our ranks who will have to pass through the same trials and difficulties, and experience the same disasters we encountered, before they will be enabled to diagnose correctly, or treat successfully any diseases, save those common to all countries, without a warning, a sign of explanation from us to guide the neophyte through the labyrinth before him. Oh! *awake*, my friends! here is an *open, unexplored path to usefulness and fame*, an object to excite ambition, commendable in its pursuit, and glorious in its attainment; the secret of whose possession is comprised in three words—Observe! Compare! Record!

The existence of a form of pneumonia, having some *undefined* connection with periodical fever, has long been known and noticed; but unfortunately for the student of the present day, the affection has not attracted the attention of recent investigators, and is only censorily and imperfectly noticed by any of the modern standard authorities.

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In my opinion the phase of disease whose delineation is now attempted, is the form of disease generally seen by Southern observers; that its peculiarities are often unrecognized, being often confounded with simple typhoid and other varieties of pulmonic lesion. Not only this, but the assertion is here made that under proper management this disease, although of decidedly fatal tendencies, is more under the control of remedial agents than any of the so-called phlegmasiæ. If these declarations are true, how important is it that its features should be familiarly known, in order that it may *not be confounded* with diseases chiefly affecting the same organs, but *so unlike in character and nature.*"

The Italics are ours—the marks of exclamation are Dr. Manson's.

3. That there exists in the South, as a distinct and different disease, a form of pneumonia, designated by him as "malarial pneumonia," about which especially very little is known, and of which the author proposes to give an account in that paper.

It is well known that this production obtained a very early and wide notoriety. Indeed, it was transferred from "the advance proof-sheets" of the Transactions of the Medical Society of this State, to the pages of the Virginia Medical Journal, and appeared there simultaneously with its insertion in the New Orleans Medical News and Hospital Gazette.* A paper of such rapid and extensive circulation, conveying charges of so grave a character against the practitioners of the South, and presenting, at length, views, unique, if not novel and imposing, in regard to the form of a disease of which most of us must have encountered many examples, would seem to demand some notice from those who do not consider themselves altogether amenable to the charges conveyed, and who differ widely from the author as to the real nature of the alleged form of disease, in the diagnosis and treatment of which especially, they are said to be so profoundly at fault. It becomes "us of the South," and more particularly the physicians of North Carolina,— to whose State Medical Society this essay was first submitted,—to give it a careful and candid study.

In some things, we are happy to agree with Dr. Manson. We agree with him, that upon this "grand theme, pneumonia," the "productions of contemporaneous writers alone" are "prolific"—if by that he means that they are numerous and extensive. We further agree with him, that the subject has been illustrated by "the vast labors and researches bequeathed by the mighty dead, the offspring of the greatest minds which have adorned our annals." But we differ materially from him in regard to the value of those productions, of those labors and researches. They appear to him to have left the subject in "*great confusion.*" It seems to us that there is no affection which has been more successfully studied, and which is more accurately known. To such an extent is this true, that he must, indeed, be a diligent

* See Virginia Medical Journal for Sept. and Oct., 1857; and N. O. Medical News and Hospital Gazette for Sept., 1857.

student, and greatly to be desired are his attainments, who can conscientiously declare, (and be himself fully aware of the entire import of the declaration,)—*I am master of what is already known:* for he who attempts to compass it all, will soon find that he has no short and easy task before him.

In no department, perhaps, of medical science, has the spirit of modern research developed so many inestimable truths, or more essentially extended the area of actual knowledge, than in that department—diseases of the chest—first so wonderfully elucidated by the matchless genius of Laennec. To him belongs the undying honor of having opened new avenues of research, and displayed almost to ocular inspection diseases that hitherto were often only guessed at during life, or not even suspected until revealed by the scalpel after death. And although subsequent and more enlarged experience has not, in every instance, verified the entire accuracy of his observations, yet he watched so closely the phenomena of disease, and displayed such masterly ability in deducing correct conclusions from them, that even at the present moment—after having been submitted to a more scrutinizing ordeal than ever tested the teachings of other men—his doctrines are, in the main, almost every where received as the just exponents of the truths of nature. Of all the diseases, the pathology and diagnosis of which that great man illustrated and portrayed, none has, perhaps, received more subsequent attention, or elicited the genius or stimulated the research of so many observers, as that of pneumonia. Of all acute pulmonary affections of grave character, it is of most general prevalence, complicates and enhances the danger of the greatest variety of disorders, and numbers the most victims.

Pre-eminent among those, who, following in the footsteps of Laennec, have studied and elucidated its history, stands by universal consent the name of M. Grisolle. It has been well said by a celebrated reviewer, that that patient and indefatigable student of nature, having constantly pursued for years the clinical study of this disease, and availed himself of every contribution to the subject, whether domestic or foreign, has produced a work (*b*), "possessing a *character of completeness, which in the*

(*b*) Traité Pratique de la Pneumonie aux différens ages et dans ses rapports avec les autres maladies aigues et chroniques. Par A. Grisolle, D.M.P., Médecin du bureau central

present state of knowledge could not have been given to the history of any other disease." And since the publication of his treatise, the literature of the subject has been still further enriched by many able contributions from the best men in the profession, both at home and abroad. So that, as it appears to us, pneumonia may be regarded, at the present time, as of all diseases, that which has been most thoroughly explored, and which is best understood. And, certainly, there is a striking contrast between the views of Dr. Manson, respecting the knowledge and skill of Southern practitioners, and those of the able and lamented Drake—"our great medical philosopher," as Dr. La Roche calls him (*c*)—who, having conversed with hundreds of physicians, scattered from the shores of the Gulf of Mexico to the Great Lakes, and from the Mississippi to the Atlantic Ocean, previous to composing his work on the Principal Diseases of the South and the interior Valley of North America, thought that the treatment of pneumonia, "is, on the whole, as successfully accomplished by our physicians as by those in the great cities of the Atlantic States or of Europe." (*d*)

Differing, therefore, with Dr. Manson on this point, we fully agree with him on another. We certainly think that any one at this day may well approach this subject with misgiving of his own ability to add to what is known. And we are not surprised that Dr. Manson should have been impressed with "mingled emotions of distrust, humility and awe,"—though scarcely for the reasons assigned by himself: for while, as he modestly states, it was neither "his aim nor expectation to produce anything striking or original," yet, in exploring the vast domain "of nature," he discovers "*an open, unexplored path to usefulness and fame*," *which* "*has not attracted the attention of recent investigators, and is only cursorily and imperfectly noticed by any of the modern standard authorities*"!

Now, we would by no means be understood as repressing further researches into this, or any other disease. In the present

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des Hôpitaux de Paris, Membre de la Société Médicale d'Observation, &c., &c.—Paris, 1841 8vo., pp. 747.

(*c*) Pneumonia: its supposed connection, Pathological and Etiological, with Autumnal Fevers, &c., by R. La Roche, M.D., 447.

(*d*) Op. Cit. 856.

state of science, our knowledge of no disease whatever can be said to be perfect; and, Dr. Manson very prettily observes—"as the lowly miner may pluck from its secret hiding place a gem worthy to deck an imperial diadem, so the humblest searcher into nature's mysteries may often elicit truths which have escaped the vigilance of his more gifted compeers." We incline to the opinion, however, that such lucky hits are, like black swans, extremely rare. And we further incline to the opinion, that amid the wonderful mental activity of these modern times, when the minds of men exhibit a restless and persevering spirit of investigation, which eagerly scrutinizes the foundation of prevalent doctrines and precepts, however hallowed by age or sanctioned by experience, and as eagerly wanders away into the wildest fields of speculation and conjecture, old familiar truths of imperishable value are often discarded or neglected for supposed brilliant novelties or fanciful conceits—innovation is mistaken for discovery—and mere commotion for progression.

But it is not to be denied that the zeal with which Dr. Manson enters upon his task, is worthy of all commendation. We admire the enlightened enthusiasm for scientific truth, and the ardent ambition for its attainment, which vivify and irradiate his pages. It is a high spirit and noble purpose which he would incite in others. And we find him accordingly inviting and exhorting his Southern brethren to its earnest and active exercise in the promotion of the great cause of medical science and improvement. The talismanic words which he commends, convey advice that challenges eulogy and compels obedience—"Observe! Compare! Record!

Gladly would we follow that wise admonition. And most happy should we have been to have marked evidences of its observance by the talented author, in the extraordinary essay before us. With great respect, however, we must say that we were not a little disappointed in finding that Dr. Manson had not, in our judgment, fortified his precept by his example; for we cannot but add, with equal candor and courtesy, that in this production we look in vain for the results of careful and recorded observations, compared with those the author might, as it appears to us, have himself collected, or with such as are amply furnished

in the investigations of others. If in the examination which we propose to make of this paper, we find that his observations and reflections do not correspond with those presented by our own experience, and with the recorded experience of others, it is due to the cause of science, as well as to ourselves, that we should accept Dr. Manson's urgent invitation, and "record" what we have "observed" and "compared." It often happens that the clash of conflicting minds emits the spark that lights up the pathway of truth.

PART I.

Dr. Manson refers to three forms of pneumonia :

1. Simple Sthenic pneumonia.
2. Asthenic or Typhoid pneumonia.
3. "Malarial pneumonia."

He thinks that "malarial pneumonia" is the form of the disease "generally seen by Southern observers ; that its *peculiarities* are often *unrecognized*, being often *confounded* with simple typhoid or other varieties of pulmonic lesion" ; but that it differs essentially from them—so much, indeed, as to be "*unlike* in *character* and *nature*"! Dr. Manson proceeds to describe the rational symptoms, physical signs, diagnostic differences, cause and nature of "malarial pneumonia"; and concludes with its treatment.

We propose, in reviewing this essay, to look into the items of the grave charge preferred against us "*of the South*"; to compare "malarial," with other divisions of pneumonia, in all its leading features, and to scrutinize closely its distinctive traits ; to enquire whether we have, in fact, been so blind as to stumble over, instead of seeing and bravely combating this alleged form of disease of peculiar characteristics, said to be so radically distinct from others in its essential nature, which, like some huge monster, has so often obstructed our path, and deprived us of the laurels that were blooming within our grasp. We shall see whether what Dr. Manson terms "malarial pneumonia", be so different from the ordinary forms of the disease, as justly to be regarded as "unlike in character and nature." We shall endeavor to prove that Dr. Manson has failed to produce suffi-

cient evidence to maintain his positions; and that what he considers as a separate and distinct form of pneumonia, is nothing more, *in the immense majority of cases, at least*, than ordinary asthenic or sthenic pneumonia, complicated by the accidental impression of the poison of malaria; and, as such, clearly described by some of the best "modern standard authorities," and, of necessity, well known to all enlightened physicians.

What, then, are the features of the disease described by Dr. Manson?

I.—

"Malarial pneumonia," says Dr. Manson, "attacks *suddenly*, being almost invariably ushered in by a *chill*, which is frequently of a severe intensity, and attended with rigors of variable duration. In very grave cases, however, the system is prostrated almost beyond the capability to react, and a prolonged cold stage is the result. Death, however, rarely is produced from this cause in the early stage, but fever, more or less developed, soon ensues. In some cases the patient seems for hours, and even days, to hover between the state of chill and febrile excitement. *Pain in the side*, of the pleuritic character, is generally next experienced, it being absent in very few instances."

In what respect does this description vary from that of ordinary frank pneumonia?

1. Prodromes. Walshe—whose treatise on thoracic affections has been pronounced by high authority (*a*), unrivalled—states that in acute frank pneumonia, the *invasion* is *sudden* in the majority of cases; but that a certain proportion of adults, varying between a *sixth and a third* of the whole number attacked, suffer from *prodromata* in the shape of general febrile disturbance, of from one to four days' duration, after the action of the cause, and before any local symptom of the disease makes its appearance. (*b*) Grisolle found that between the ages of sixteen and sixty, about *one-fourth* have precursory symptoms. Thus, of two hundred and one patients whose antecedent history he was enabled to obtain, seventy-six had coughed during a variable period before characteristic symptoms of pneumonia were developed. Of these latter cases, twenty-three had suffered for years from chronic bronchitis; and fifty-three had presented evidences of bronchitis for three or four weeks before the attack of pneumonia. Excluding the twenty-three cases of chronic bronchitis, these observations exhibit fifty-three. who had acute bronchitis

(*a*) Da Costa, Med. Exam., Vol. X, 673.

(*b*) Prac. Treat. on Dis. of Heart and Lungs, London, 1854, 421—2.

before the pneumonic seizure—a little more than *one-fourth* of the two hundred and one cases. (*c*) Wood states that not unfrequently the disease commences with catarrhal symptoms, which continue for several days before signs of pneumonia become manifest. (*d*) Valleix repeats Grisolle's statement, and says that the prodromes generally consist in a feeling of malaise, diminution of appetite and strength, of dejection, lumbar pains, a very great sensibility to external cold, and erratic shiverings. (*e*)

2. The *onset* is marked by a *chill* with greater frequency than in any affections, except ague, and perhaps puerperal fever (Grisolle, Walshe). In 79 cases observed by Louis, 61 presented shivering, of more or less intensity, during the first day; and oftener still it marked the beginning. In 63 out of 71 cases seen by Chomel, it announced the commencement. In 182 cases in which Grisolle sought this phenomenon with care, it existed in 145 in the first twelve hours; and in 110 (nearly *four-fifths*), it indicated the onset. Its *intensity* was variable. In 103 cases in which this point was noticed, it was violent in 46—so much so, that the body was agitated by tremblings, often with chattering of the teeth, as in the shiverings of intermittent fever in the first stages. In 20 instances it was slight—there were horripilations—and recurred frequently during the first days of the affection. The *duration* varied from fifteen minutes to three hours. (*f*) Gerhard says that it lasts half an hour or more, and sometimes two or three hours (*g*); and Copland makes the same statement. (*h*) In June, 1844, we treated a woman 56 years of age, in which it continued for *five* hours, and was succeeded by great dyspnœa, intense fever, and a full, strong pulse. The disease occupied the left summit, and yielded only to the most active antiphlogistic treatment. Recently we attended an athletic carpenter, who remained in a cold and shivering condition for *eight hours* after the first access of chill, before the beginning of the fever. The *shivering* of pneumonia is remarkable, says Chomel,

(*c*) Traité Pratique de la Pneumonie, 182.
(*d*) Prac. Med., Vol. II, 9—4th edition.
(*e*) Guide du Médicin Praticien, Tome I, 430.
(*f*) Trait. Prat. de la Pneum., 190.
(*g*) Diseases of the Chest, 2nd Ed., 146.
(*h*) Dictionary of Practical Medicine, Amer. Ed., Vol. II, 879.

inasmuch as it is very intense, and of long duration. Occurring after the age of from 55 to 60, it *almost always* announces the onset of pneumonia; and this may be accompanied by bilious vomiting, or more or less disturbance of the mind, without diminishing its semeiotic value. One form of the initial shivering, he adds, may give rise to considerable embarrassment, namely, when a second attack occurs 24 or 48 hours after the first, which, when the ordinary symptoms of pneumonia are absent, may easily lead to the belief of the existence of an intermittent. This double shivering is not very rare, and may be due to the successive development of the disease in the two lungs. (*a*)

3. *Pain in the side*, as every one knows, is one of the most frequent symptoms of pneumonia. It appeared in 161 of 182 cases observed by Grisolle within the first twelve hours from the time of seizure. Dr. H. A. Ramsay, of Georgia, describes a form of pneumonia, of which he saw about 370 cases, the cause of which he attributes to "malaria, and the agents which promote its production,—atmospheric deviations and vegetable decomposition." In 170 of these cases, *pain in the side* was noted as absent in only *fifteen*. (*b*)

It appears, then, that in regard to the *suddenness* of the *invasion* in the large majority of cases, the existence of *prodromata* in a certain number, and the occurrence of *chill* and *pain in the side*, the so-called "malarial pneumonia" offers no peculiarity in the mode of attack in any respect whatever. That ordinary acute frank pneumonia generally commences *suddenly* with a *chill* and *pain in the side*, is the universal experience of the best observers. (*c*)

II. Dr. Manson proceeds:—

"To the reactive fever dyspnœa and cough soon succeed. In some cases, however, the cough is tardy in its appearance, or is so slight at the commencement as to elude the notice of the patient and physician. Headache, nausea and vomiting are very frequent and prominent symptoms, and frequently precede the cough and pain; the fluids ejected from the stomach being usually of a bilious character. The cough is commonly dry at

(*a*) Brit. and For. Med. Chir. Rev., July, 1850, 259–60.
(*b*) Charleston Med. Jour., Vol. VI, 1–26.
(*c*) Gerhard. Op. Cit., 142–6; Swett, Dis. of Chest, 88–9; C. J. B. Williams, Cyc. Prac. Med., Vol. III, 590; Watson, Lec. Prac. Phys., 566: Andral, Med. Clinic, Vol. II, 184–5; Barlow, Prac. Med., 188–9; Wood, Prac. Med., Vol. II, 8–9; Dickson, Elements of Med., 605; Drake, Op. Cit., 854, &c.

the commencement, or attended with the excretion of glairy mucus; sooner or later, however, the sputa becomes (become?) more or less tinged with blood, forming the pathognomonic ferruginous expectoration, the hues of which are as numerous as the proportions are in which blood and mucus may be mingled. Thus we shall see in some cases the latter merely tinged with blood; or as more frequently happens, a uniform thin, bloody, frothy fluid is only seen, possessing but little viscidity or tenacity, whilst in some instances a pure and copious hæmoptysis is observed. Later, however, the expectoration is liable to great variations in color and hue, thus it may be of the various shades of brown, red and yellow commingled, and in the advanced stages may become green, or almost black. * * * * The breathing is usually irregular on account of the pleuritic pain, but when this is absent, or has been relieved, the respiration is often but slightly embarrassed in some cases in the early stage. The disease continuing unchecked, the paroxysms usually become less marked, the respiration becomes more continuously frequent and difficult, the acts often exceeding 60 per minute; yet, in some cases of grave character, the breathing is scarcely accelerated during the whole course of the disease."

Under this head, we shall first notice the *dyspnœa*. As is well known, this is not peculiar to any form of lung disease, acute or chronic, and occurs in all. "The dyspnœa produced by pneumonia," says Watson, in speaking of the ordinary frank form, "varies greatly in degree in different cases; sometimes it is so slight that the patient is not conscious of it, and the physician scarcely perceives it; sometimes it is so extreme, that the patient, entirely regardless of what is going on about him seems wholly occupied with respiring" (*a*) Laennec states that *dyspnœa* is often hardly perceived by the patient, although the frequency of the respiration points it out to the physician; in some cases it is not more frequent than in health. (*b*) According to Walshe, the number of respirations vary from 24 to 80 per minute: from 30 to 50 being the usual extremes, and the *frequency* of the breathing does not depend on any particular seat, nor, singularly enough, even necessarily on the extent, of the pneumonia. Double pneumonia may not accelerate the respiration as much as inflammation of a limited portion of one lung. (*c*) We ourselves have seen a case of double pneumonia, during our term of service as one of the resident physicians in the Baltimore Alms House Hospital, with a respiration not exceeding 20, and with a pulse of 72, per minute. "In consequence of unaccountable idiosyncrasy," says Andral, "there are some indivi-

(*a*) Op. Cit., 507.
(*b*) Diseases of the Chest, Amer. Ed., 1838, 228.
(*c*) Op. Cit., 422.

duals, a very small portion of whose lung is in the first stage of inflammation, and whose breathing, nevertheless, is very much embarrassed. There are others, in whom a much greater portion of the lung is inflamed in the second or third stage, and who still feel much less dyspnœa. (*d*) When the dyspnœa is excessive, however, it *usually* indicates that a considerable extent of lung substance is invaded: indeed, especially during the earlier stages, it is a tolerably accurate index of its amount; but to this rule, exceptions are not of very infrequent occurrence. When the whole of one lobe of a lung is involved, according to Gerhard, the frequency of the respiratory acts is increased to forty or fifty a minute. It can scarcely be necessary to add that as *irregularity* of the breathing, when present in "malarial pneumonia." is said by Dr. Manson to depend entirely on the existence of *pleuritic* pain," there can be nothing peculiar to the malarial form of the disease in that circumstance.

We would invite attention to the fact—one of importance for diagnosis—alluded to by Carpenter (*e*), and particularly insisted on by Walshe, that in pneumonia. the *normal* ratio between the *pulse* and the *respiration* is often strikingly perverted. Thus, for 100 pulsations per minute, there may be 60 respirations—giving a ratio of 1-7:1, instead of 4-5:1, that of health. We have had recently under care a case in which the entire right lung from summit to base, was invaded. About the acmé, the pulse was 108, and the respirations, 72 per minute—presenting a ratio of 1-5 : 1—which, so far as our knowledge extends, is the most remarkable upon record. It occurred in a robust mulatto, about ætat. 35, whose normal pulse was ascertained after recovery to be about 70. Now, in no other disease is the perversion of the physiological ratio between the pulse and the respirations so prominently manifested. In pleurisy, although during the acute period the respiration is very constantly accelerated, yet, as Walshe observes, the acts very rarely exceed the *third* of the number of the heart's pulsations; and in capillary bronchitis, the pulse and respiration are both very frequent, and out of proportion, but never so much so as in some cases of pneumonia. (*f*)

(*d*) Op. Cit., Vol. II, 186.
(*e*) Elements of Physiology, 389.
(*f*) Walshe, Op. Cit., 293–422.

Dr. Manson takes no note of this perversion of the pulse—respiration ratio ; and we cannot, therefore, institute a comparison in this respect, between his "malarial", and the simple uncomplicated form of pneumonia. But, we will mention, *en passant*, that it is often observed of marked character in pneumonia imbued with a malarious influence ; and we have frequently anticipated the occurrence of rust-coloured spatum and the appropriate physical signs early in the attack of a central pneumonia, by the very remarkable perversion of the ratio which normally subsists between the pulse and the respiration.

In regard to the *character* of the *dyspnœa*, we will remark that, those who have studied this phenomenon with care, must have noticed, with Gairdner, of Edinburgh, that in *pure* pneumonia, it is a mere *acceleration* of the respiratory movements, unaccompanied by the heaving or straining inspiration, so generally observed in severe capillary bronchitis. A patient suffering from an uncomplicated pneumonia of great extent, involving even the whole of one lung, increasing the acts to 60 in the minute, and producing a certain degree of lividity of the face, may, notwithstanding, as we have seen, often lie quietly in bed, without any of that uplifting of the shoulders, and urgent disposition to assume the erect posture, which render a severe attack of suffocative bronchite a spectacle so distressing to behold.

We conclude, therefore, that there is nothing peculiar in the *dyspœna*, or the *frequency* of the respiratory acts—both being liable to great variations, alike—in "malarial," and the simple forms of pneumonia.

2. The *cough* need not detain us long. Dr. Manson says little about it, doubtless on account of its being so common a symptom in pulmonary affections, and its offering nothing very peculiar to the disease under discussion. In "malarial pneumonia" it "soon succeeds the reactive fever" ; "is commonly dry at the commencement, or attended with an excretion of glairy mucus; sooner or later, however, the sputa" assume their "pathognomonic" characters. Watson says that in simple frank pneumonia, it has no particular character, and affords but little information ; that it does not usually take place in paroxysms, and its severity and frequency are not always proportioned to

the extent and intensity of the inflammation ; that it is generally dry in the outset, but in a few hours is accompanied by the expectoration of peculiar sputa. Dickson alleges that " its sound is peculiar ; not abruptly suppressed like that of pleurisy, nor vehement like that of bronchitis, but anxious and unsatisfactory." Gerhard describes the pneumonic cough as being short and suppressed, owing partly to the pain felt during the act of coughing, and partly to the impossibility of completely inflating the lungs. Walshe states that it is moderate in amount, and occurs in the majority of cases during the first twelve hours. It is highly important to remember that, in some cases, it is wanting throughout the entire course of the disease. But in no respect whatever, does it present any character at all distinctive of " *malarial* pneumonia."

3. The *expectoration* offers great interest. Andral long ago demonstrated that its characteristic appearances are due to the varied inter-mixture of mucus and blood ; and his views have been generally adopted by subsequent writers. " If," says he, " we mix with pure water, rendered viscid by means of mucilage, a little blood, the proportion of which is gradually increased, we see it successively become tinged with a deeper and deeper yellow, then with a greenish yellow, then with a yellow which is confounded with red, whence the iron-red colour, then, in fine, with an intense red." (*a*) Hence it is, as Walshe describes, that the sputa are sanguinolent, or rusty in tint, viscid, semi-transparent, adhering to the vessel, but slightly aërated, passing through various shades of orange, apricot-colour, faintly greenish, etc. In some cases the sputa are diffluent, watery and of dull brown, or even faintly blackish hue,—resembling liquorice juice, or prune juice—conditions of evil augury. (*b*) Now Dr. Manson admits that the " pathognomonic ferruginous" variety is the type character of the sputum in pneumonia, and that it is often seen in the malarial form ; yet more frequently in the latter there is, he says, " a uniform, thin, bloody, frothy fluid, possessing little viscidity or tenacity ; in fine, presenting more the aspect of a defibrinized blood than as a product of inflammatory action."

(*a*) Op. Cit., Vol. II, 194-5.
(*b*) Op. Cit. 423.

But an important question arises here: is this kind of expectoration in any degree *diagnostic* (as Dr. Manson alleges) of "*malarial* pneumonia"? Is it not often seen in cases that offer not the slightest evidence of a malarious impression? One of the ablest writers on practical medicine, thus describes the sputa in *typhoid* pneumonia, which is one of the the types of pneumonia that Dr. Manson says is often "*confounded* with the "malarial" form, but which should be carefully distinguished from it, since they are said to be "*so* unlike in *character* and *nature*":—in *typhoid* pneumonia, says Prof. Wood, a "very striking *peculiarity* is the character of the matter expectorated. Even in the early stage, it is generally bloody, and sometimes almost pure blood. In all stages, it is *less viscid*, and usually more copious than in ordinary pneumonia. Not unfrequently it is brown or even blackish, and sometimes fetid. Either from want of energy in the vessels, or from the character of the circulating blood, there appears to be an *oozing of this fluid, little changed, through the capillaries, instead of the somewhat plastic lymph that is exuded in vigorous inflammation.*" (*c*) Indeed, even in the sthenic form of the disease, Prof. W. affirms that, "sometimes instead of the characteristic *viscid* sputa, we see a *copious* expectoration of a *uniform liquid, like mucilage*, which is more or less *tinged with blood*." (*d*)

It appears then, that according to high authority, the expectoration in question is so often related to *typhoid* pneumonia as to constitute a "*very striking peculiarity*" of that type of the disease; and yet Dr. Manson classes it among the *diagnostic* signs of the "*malarial*" form! But if the latter is not to be "*confounded*" with the former—from which we are told, it differs "*so*" radically—it follows, of necessity, that since the expectoration is often alike in both, *it* cannot be distinctive of either; that between these forms of pneumonia, it utterly fails as a differential diagnostic.

We conclude, therefore, that as, according to Dr. Manson himself, this thin, bloody, slightly tenacious, bloody expectoration occurs only in the majority of cases of "malarial pneumo-

(*c*) Op. Cit., Vol. II, 19.
(*d*) Ibid., 10.

nia," and is absent in many cases undoubtedly of that character; and according to Dr. Wood presents a "very striking *peculiarity*" of *typhoid*, and may occur even in the sthenic form of pneumonia,—either of which often prevails in localities where periodic fevers are never known to originate,—it is without value in the diagnosis of pneumonia impressed by the poison of malaria; and that there is no special character of the sputa, peculiar to, or diagnostic of, that variety of the disease.

In regard to there being "a *pure* and *copious hæmoptysis*" in pneumonia, we agree entirely with Walshe, that if the expectoration be actually more or less profusely bloody—if there be *hæmoptysis* in short—the pneumonia is connected with the deposition of tubercles. If it ever exists in pneumonia uncomplicated by tuberculous deposit, it must be excessively rare in this section of country; for in a practice of nearly fifteen years in this region, we have never seen *hæmoptysis* in pneumonia amounting even to a wine-glassful, that was not associated with the physical signs of phthisis, or in which the future progress of the case did not soon afterwards present them, save in a single instance occurring in a woman about seventy, who had long-standing organic disease of the heart.

Headache, nausea, and *vomiting.* These symptoms are said by Dr. Manson, to be "very frequent and prominent" in "malarial pneumonia,"—"the fluids ejected from the stomach being usually of a bilious character."

4. *Headache* is a "very frequent and prominent" symptom of most acute affections of a febrile and inflammatory character, and is by no means infrequent in trivial disorders. Pneumonia, of whatever form, offers no exception to this law. Walshe states that in the ordinary acute form, *cephalagia* is of frequent occurrence, and comes on with the outset, as part of the febrile state. According to Wood, the fever is very often attended by *pain in the head*, especially about the brows or forehead, from which the patient may even suffer more than from the pain in the chest; and occasionally, the *headache* is the only symptom of which the patient complains for the first few days. (*a*) Dickson

(*a*) Op. Cit., Vol. II, 11.

mentions the occurrence of *headache*, with pains in the back and limbs, and nausea even to vomiting. (*b*) Other authors, also, notice the presence of *headache* as super-vening upon the reactive fever. (*c*) It was absent in only 8 out of 57 cases observed by Louis; commencing at the onset, it ceased after the eighth day. (*d*) Grisolle regards *cephalagia* as among the most frequent sympathetic phenomena, having found that in 125 cases in which it was sought with care, there were only 20 which never presented it at some time during the sickness. In the vast majority of cases, it occurred in the beginning—there being only three exceptions to this rule in his experience. In *five-sixths* of the cases, it manifested itself in the forehead. (*e*) *Headache*, then, is common alike to simple, and "malarial pneumonia."

5. *Nausea*, and the *vomiting* usually of bilious matters, are spoken of by Dr. Manson in his description of the symptoms that mark the "general course and progress" of "malarial pneumonia," as being "*very frequent and prominent*"; but in the section on "Diagnosis," he points out their "frequent presence in *several* cases," as affording "*some aid* to diagnosis,"—and states that although "the evidences of hepatic and gastric disorders have gained for the malarial form the epithet of '*bilious*,'" yet he "utterly rejects that view of its pathology, which associates either this disease or any form of malarial disease necessarily with hepatic lesion." In ordinary simple pneumonia, *nausea* and *vomiting* in *adults* are not very often met with, unless when provoked by medicines. Walshe speaks of occasional vomiting. Wood notices it as an occasional but accidental symptom. Swett thinks it not uncommon for a patient to vomit at the commencement of the disease, especially during or after the chill. Williams states that under the influence of the reactive fever, the tendency of particular constitutions, or the morbid disposition of individual organs, is discovered; and that in some the gastro-enteric mucous membrane especially feels the effect of the

(*b*) Op. Cit., 605.
(*c*) Barlow, Op. Cit., 188; C. I. B. Williams, Op. Cit., Vol. III, 590; Drake, Op. Cit., 855; Copeland, Op. Cit., Vol. II, 880.
(*d*) Anatomical, Pathological and Therapeutical Researches, etc., on Typhoid Fever, by P. Ch. A. Louis, Vol. II, 114.
(*e*) Op. Cit., 296.

vascular excitement, evincing thirst, epigastric tenderness, irritability of stomach, and the various sympathetic signs of gastric fever; while in others there are bilious vomiting, turbid, saffron-coloured urine, and occasionally a jaundiced tinge of the skin.(*a*) Grisolle noticed the occurrence of *vomiting* in *one-fifth* of his cases—being observed in almost all in the first three days of the affection. If the pneumonia began soon after eating, the alimentary materials were ejected more or less altered; but in nearly all the others, the vomiting was of a bilious nature. (*b*) It is well known, indeed, that in most diseases, should vomiting from any cause happen to be somewhat protracted, and especially should it occur while the stomach is empty, that bile will be ejected. It would, therefore, be illogical hastily to conclude in all such cases, that the liver was primarily at fault; for the discharge of bile may only be an evidence that emesis has occurred with retching—the inverted action of the stomach being, as Dunglison suggests, extended to the duodenum, and the irritation produced in the alimentary canal, being propagated along the biliary ducts to the liver, so that the secretion from it becomes augmented. And even in those instances where there is bilious vomiting from the outset, that does not necessarily imply a *malarial* impression; for bilious vomiting occurs in pneumonia where there is no positive and conclusive evidence of the existence of that element in the case. We have seen such instances repeatedly, and especially in the Spring, when gastro-intestinal affections and functional disorders of the liver are very common in this region, and complicate the phenomena of various diseases. And, on the other hand, it is equally true, as Dr. Manson has stated, that there is no necessary association of hepatic lesion with pneumonia undoubtedly impressed by a malarious influence. Hence it is that Drake, in defining the phrase bilious or malarial pneumonia in his section on "Bilious Pneumonitis," says that he employs it—although it is objectionable and arbitrary—for mere convenience, to express the presence and action of the diathesis peculiar to autumnal or periodic

(*a*) Williams, Cyc. Prac. Med., Vol. III, 610; Swett, 104; Wood, Vol II, 11; Walshe, 425.
(*b*) Op. Cit., 268-9.

fever; and that is not applicable to every case in which pneumonic inflammation is connected with disturbance of the biliary function: for the liver may be disturbed in its secretion and excretion by sympathy with some other organ; nor does it always indicate a disease attended by bilious vomiting, for in many cases, undeniably connected with the autumnal or periodic fever diathesis, no bilious symptoms may be present. (*c*) It is admitted on all hands, that bilious derangement occurs in pneumonia when an irritant or inflammatory action extends from the lower lobe of the right lung to the liver, and when pneumonia by the same law of contiguity, super-venes upon hepatic disease. But, notwithstanding all these considerations, persisting nausea, or marked irritability of stomach, bilious vomiting and other evidences of hepatic trouble much oftener present themselves in pneumonia offering positive proofs of the mingling of a malarious element, than when no such proofs are present; and consequently, when taken in connection with other phenomena, should arrest attention as among the corroborative tokens of that special influence.

III. Condition of the *bowels*, and the character of the *stools*. Under the head of "Diagnosis," Dr. Manson directs special attention to the state of the *bowels*, and the appearance of the *discharges*. He states that—

"In the malarial form, the intestinal canal seems generally to be in a state of phlogosis or exalted sensibility. This is manifested by the frequent presence of diarrhœa; and when this is not evident, by the hypercatharsis often induced by the mildest aperients. Later, when the disease succumbs, convalescence is usually announced by the critical discharge of greenish, black, viscid, inodorous stools, closely resembling tar in appearance."

1. It should be remembered that the *intestinal mucous membrane* is generally much more impressible in the South in all diseases than it is at the North. This important pathological law is well understood by the intelligent practitioners of this section of country. Dr. Bolling, a distinguished practitioner of Montgomery, Alabama, in an able essay on Remittent Fever, remarks —"it is an erroneous impression among physicians who have not practised in the South, that more powerful and active purgatives are requisite here, and one productive of much mischief, as

(*c*) Op. Cit., 868-9.

causing the Northern practitioner settling in the South to double the size of his purgative dose, when its reduction one-half would probably be efficient, and certainly more safe. On the contrary, the *bowels are extremely susceptible to the action of purgatives, which, without great caution, are apt to produce much irritation and hypercatharsis.*" (*a*) Hence it is, as Drake affirms, that drastic purging is held to be injurious in the malarious fevers of the South and West, and that the change of practice in this respect, in that region, has been signal and decisive. And Dr. Ames, also, an eminent physician of the same town with Dr. Bolling, in an ingenious article on the treatment of pneumonia, says—"it is well known that a *state of irritation of the intestinal or gastro-intestinal mucous membrane*, and of functional excitement in the liver, are *almost uniform conditions of our endemic disease.*" These conditions are indeed *so general*, that we may very properly conclude their production to be a *law* of the remote causes, whatever may be their nature, of the *most important of our acute endemic diseases.*" In speaking of the facility with which the usual toxicological effects of mercury and antimony are induced on the gastro-intestinal mucous membrane, and on the functions of the liver, he adds—"they have been observed to occur much more frequently in pneumonia, *in this climate than in some other climates;* and, in this climate *much more frequently in pneumonia than in some other diseases.*" (*b*) It may be remarked, however, in regard to the latter statement, that there is not, perhaps, any disease so actively treated, as a general rule, in the South, as pneumonia. The vast majority of cases are pretty heavily dosed with calomel and tartar emetic—a combination, as we shall hereafter see, particularly apt to light up gastro-intestinal irritation, and which sometimes proceeds to a most destructive inflammation. Often in the career of most of our diseases, the

(*a*) Amer. Jour. Med. Sci., N.S., Vol. XII, 24. Dr. B. makes the observations quoted above, while speaking of the employment of purgatives in *remittent fever;* but it is clear to us, that they are general in their scope and meaning, and intended to apply to all diseases in the South. If any doubt should arise on this point, it may be removed by reference to a remark of his in a subsequent article on the deleterious effects of tartar emetic on the intestinal canal in pneumonia. In referring to this influence he says:—"Owing to the fact, that in the North, the *susceptibility of the gastro-intestinal mucous membrane to irritant impressions is less than in the South,* it probably occurs less frequently there than with us." *Ibid.*, Vol. XXII, 333.

(*b*.) New Orleans Med. and Surg. Jour., Vol. X, 419.

bowels are inclined to give way, owing to the natural impressibility to the action of cathartic medicines, or such as irritate the intestinal canal, which the case may have required; and we have noticed that death is not unfrequently preceded by more or less profuse alvine dejections, save a few hours before dissolution, when, in consequence of the blunted condition of the nervous system, irritating impressions are no longer perceived. Many cases of pneumonia—particularly those occurring in the Spring—offer this (in our climate) normal susceptibility very highly marked; and that too, in instances not at all complicated by a malarious impression. To assume that this state of the bowels is owing to the action of that element, in the absence of any demonstrable proof of its existence, is simply to beg the question. In bilious cases, as manifested by nausea and the vomiting of bilious matters, and icterode condition of the eyes and skin, bitter taste in the mouth, etc., the bowels are usually still more impressible; and this is true in our experience, whether the case is complicated by a malarial taint or not. As the instances attended by bilious disturbance are oftener met with in the "malarial," than in the simple uncomplicated forms of pneumonia, excessive sensibility of the bowels to the action of irritants may be somewhat more common in the former; but "diarrhœa," or "a state of phlogosis or exalted sensibility of the intestinal canal" are not so "generally" and exclusively observed, as to elevate these conditions to the position of a uniform pathological law, peculiar to that class of cases. There is no reason *a priori*, known to us, why a malarious influence should, necessarily, impart to pneumonia unwonted irritability of the bowels, or engraft upon it "a state of phlogosis" of the intestinal mucous membrane. In ordinary remittent fever in this latitude—and this is the form of periodic fever which Dr. Manson describes as being associated with "the signs and symptoms of the thoracic lesion," constituting the disease termed by him "malarial pneumonia"—the bowels are not *generally* very irritable, and usually require as the first step in the treatment, the administration of purgatives. It is very true, that the disease may occasionally (though rarely) set in with some degree of diarrhœa, or even like an attack of cholera morbus; and in certain years, dysentery

is not an unfrequent accidental complication. In those special cases, termed *gastric* by writers, from the predominance of abiding nausea and vomiting, intense thirst for cold drinks, epigastric pain and unusual tenderness upon pressure, etc., the bowels are very apt to be (though by no means always) extremely irritable, and to throw off large, watery, greenish, or reddish discharges, resembling the washings of flesh (*lotura carnium*). Indeed, there is no disease, perhaps, which presents a greater diversity of forms in different localities, or in the same localities in different years; but in the vast majority of mild or moderately severe cases, our experience confirms the statement made by Bartlett, that "the bowels are, to say the least, generally free from any considerable degree of irritation, so far as this is indicated by pain or diarrhœa." (*a*) Wood states that "the bowels are usually constipated, especially in the early stages," though he has "seldom found much difficulty in procuring free alvine evacuations." (*b*) Dickson—who must have seen a multitude of cases in and about Charleston, S. C.—affirms that "the bowels are generally constipated, and in the majority of instances moved with some difficulty." (*c*) Other authorities describe the bowels as being more or less torpid and constipated in the general run of cases. (*d*) Thus, Dr. Stewardson, in seventeen cases seen by him at the Pennsylvania Hospital, observed that the bowels were generally costive, the purgative medicines which were given rarely occasioning hypercatharsis; and of thirty-four cases occurring in the New York Hospital, Dr. Swett reports that diarrhœa was absent in almost every case; all the patients required purgatives at some period of the disease, and they usually acted freely without leaving any symptoms of irritation behind them. (*e*) Morehead found diarrhœa in only 6 of 114 clinical cases occurring in the natives of India. (*f*) Dr. Manson himself admits that, in the early stages of remittent fever "as it has generally appeared" amongst us, "the bowels are *generally*

(*a*) Fevers of the United States, 319.
(*b*) Op. Cit., Vol. I, 269.
(*c*) Op. Cit., 228.
(*d*) Brown, Cyc. Prac. Med., Vol. II, 220; Copland, Op. Cit., Vol. I, 1,101; Condie, Note to Watson's Prac., 966; Cartwright, Elea. Prac., Vol. Prac., Vol. I, 133, etc.
(*e*) Bartlett, Op. Cit., 319-20.
(*f*) Clinical Researches on Disease in India, by Charles Morehead, M.D., Vol. I, 154.

costive, *occasionally* alternating with diarrhœa;" and that it is "*as the disease advances*" that "evidences of organic irritation, usually referable to the abdominal organs, become developed," when, *inter alia*, "aperients of the mildest kind are prone to produce intestinal spasm and hypercatharsis," and diarrhœa may now make its appearance, but *generally* its occurrence is postponed to a *more advanced period" still.* (*g*) In the remittent fever of South Alabama, Dr. Bolling thinks that the tendency to torpidity of the bowels generally described by authors, does not exist; that gastro-enteritis is present in nearly all the cases, and when it is not, that the disposition to it, and the slight causes necessary to produce it, render the utmost caution necessary in the administration of purgatives. (*h*) But it is very important to remember, as Bartlett observes, that Dr. Bolling expressly excludes from his description, the entire class of mild or moderate cases; and, moreover, being well aware of the great diversity of forms presented by this disease, at different times and in different places, he concludes his essay by stating that, all his remarks are made in reference to the disease as it prevails in his section, and that the symptoms, treatment, and post-mortem appearances, "*may all vary in different localities.*" Hence his closing words: *Romæ scribo et in ære romano.*" Johnson—whose opportunities for observation were ample—states that in the bilious fever of hot climates, "the bowels are *almost always constipated*, or in a state of dysenteric irritation," as "no such thing as natural stools in this fever are ever to be seen, unless procured by art"; (*a*) and the experienced and sagacious Chapman, represents the "bowels as *torpid*, unless *they* be involved in the affection." (*b*) Now, it would be just as logical to say that constipation is an evidence of a malarious impression, as that a state of dysenteric irritation is a token of that influence, and even more so: for it is plain that the former condition is regarded as the most frequent. And the general tenor of

(*g*) See Dr. Manson's paper on Remittent Fever in the Virginia Medical and Surgical Journal, for January, 1855; also, the Transactions of the Seventh Annual Meeting of the Medical Society of North Carolina, May, 1856, in which it was subsequently republished.

(*h*) Amer. Jour. Med. Sci., Vol. XII, 24.

(*a*) Tropical Climates, 91.

(*b*) Benedict's Compendium of Chapman's Lectures in the University of Pa., 43.

authority, collected (as we have just seen) from the writings of various observers in different and widely-distant regions of the globe, is certainly not on the side of those who contend for "a state of phlogosis" or "special exalted sensibility of the intestinal canal" as one of the uniform pathological characteristics of remittent fever.

If we should now turn to the *necrotomy* of this fever, we would find that, although its pathological anatomy has not been as clearly made out, and is not so well established as that of some other essential fevers—owing, perhaps, in a great measure, to the diversity of lesions presented in different endemics—yet, if we mistake not, there might be found very good reasons for concluding with the lamented Bartlett—who was perfectly familiar with the literature of the subject—that there is no satisfactory evidence that the intestinal mucous membrane is the seat of any constant, important, or characteristic alteration; no evidence that it is more than occasionally and accidentally altered, —the changes met with after death being only those customary and accidental lesions, found with the same frequency in many other acute febrile affections.

We conclude, therefore, that while (as already stated), all diseases at the South are prone to evince greater susceptibility of the intestinal canal to irritating impressions than they usually exhibit in colder latitudes, and that pneumonia offers no exception to this general law, yet we are not warranted in considering this tendency as being peculiar to, connected with, or in any manner distinctive of, the particular diathesis of periodic fever.

2. Special character of the *intestinal discharges*. These come too late for useful diagnosis, and, we suppose, are only adduced by Dr. Manson to complete the list of symptoms which he deems differential between "malarial," and other forms of pneumonia. In the former, we are told, "convalescence is *usually* announced by characteristic critical alvine evacuations," which are "greenish, black, viscid, inodorous, and closely resembling tar in appearance."

In certain cases of *remittent fever*, generally termed by authors *hepatic*, on account of the marked evidences of congestion and torpor of the liver, these peculiar dejections are said particu-

larly to occur, and frequently to herald convalescence: hence they are thought to be in some measure critical. But in the immense majority of cases of ordinary remittent fever, they are not observed at all; and by some of our most authoritative writers of large experience, are not even mentioned. In his essay on remittent fever, already referred to, Dr. Manson himself does not speak of their occurrence in his description of the usual course and progress of the disease; and only when indicating the diagnosis between it and typhoid fever, does he allude to them, and then only to say that they "almost certainly denote a favorable termination in" the former. But the question arises at once—*how often are they witnessed?* We have long believed that they frequently occur from the administration of mercury alone; and in several instances we have proved it by withdrawing that agent. We do not hear of them so often now in our periodical fevers as we did formerly, owing to the administration of quinine early in the disease, and the more restricted and rational employment of mercurials: hence our inference, that they frequently resulted from the special action of these agents on the liver. Dr. Bolling considers these discharges as generally, but "not in all cases the product of calomel;" and says that when they "take place, those physicians who are most attached to it, most familiar with its use, and who use it here with most success, always relax in its administration or suspend it altogether." (*c*) Dr. Chapman taught that the extravagant use of mercury by many of our practitioners, in the treatment of autumnal fevers and other diseases, must be assigned as a frequent cause of the chronic hepatic affections in certain portions of our country (*d*); and we ourselves have seen genuine dysentery in non-malarious maladies produced by calomel—the evacuations being composed of thick, greenish, or black, chopped spinach-looking substances, inter-mixed with mucous and blood. The celebrated Irish surgeon, Colles, in his Lectures on Surgery, alludes to the dysentery induced by the use of mercury in the

(*c*) Amer. Jour. Med. Sci., Vol. XII, 35–36.

(*d*) Lec. on Thoracic and Abdominal Viscera, 326.

treatment of syphilis, and gives directions for the management of such cases. (*e*)

Dr. Manson says that those critical discharges which usually announce convalescence in "*malarial* pneumonia," are "*inodorous!*" As he seems to have often met with them, he certainly ought to know; but they are not always odorless, as we have good reason to be confident. We have seen them so offensive as to taint the apartments, affect the patients themselves with the most distressing nausea, and, even drive the attendants out of doors. Bolling, Cartwright, Condie, Eberle, Stewardson and Wood, all speak of these peculiar stools as heralding convalescence in certain cases of remittent fever, but are silent, in regard to their being destitute of odor, while the observant Chapman states positively, that they are "*fetid.*"

If Dr. Manson could succeed in proving that there is a "form of pneumonia *generally* seen by Southern observers," in which "convalescence is *usually* announced" by a particular kind of "*inodorous* stools," he will have taken an important step towards making out his case; for we are not aware that such an "affection" has "attracted the attention of recent investigators," or that it has been even "cursorily and imperfectly noticed by any of the modern standard authorities." We can hardly avoid the belief, however, that if he will use calomel less freely, and accurately note down his cases as they come under care, the number of instances of this peculiar disease will soon begin to diminish by degrees, and finally become of only rare and exceptional occurrence.

IV. Character of the *febrile movement, and of the pulse.* Dr. Manson proceeds to state that in "*malarial pneumonia*"

"The febrile action presents decided peculiarities, being usually distinctly paroxysmal and remittent. The remissions are, however, sometimes obscure, and an active fever exists during the whole day and night. On watching the patient closely, however, a coolness of the extreme parts will be almost invariably observed, generally occuring daily and during the morning hours. This reduction of temperature may be confined to the ends of the nose, toes and fingers, either separately or combined, and in grave and rare cases, may extend over the entire extremities, or even the whole surface, amounting to a state of collapse. This coolness of the skin is not generally felt by the patient, but on the contrary, when it is present, he often complains most intensely of sensations of ex-

(*e*) Lectures on Surgery by A. Colles, 347. Dr. Byford, of Ind., has a very sensible paper on Mercurial Muco-enteritis in the Amer. Jour. Med. Sci., No. LXVI, N. S., 313 et seq., to which we would refer the reader.

cessive internal heat, soliciting the introduction of cold air into the chamber; and as we have witnessed, wishing the doors and windows to be opened, and the fires extinguished, when the thermometer stood below the freezing point near the bed of the sufferer. A marked increase of fever follows this insensible chill, which continues to increase in intensity, until an exacerbation more or less perfect, is developed. The pulse which had been very frequent in the chill, becomes expanded, acquires force, and sometimes, though rarely, becomes full or tense. The term *compressible* applies to the usual condition of the pulse, and the idea entertained on its careful examination is, that the heart is acting with only a seeming force, and not with real vigor. In some few cases, the pulse has been observed to be "*nearly normal*, or *morbidly slow*."

1. The paroxysmal and periodical recurrence of the febrile movement is, undoubtedly, *the leading distinguishing characteristic* of pneumonia, impressed by a malarious influence; but it is highly important to remember that, in simple uncomplicated pneumonia, as well as in all other febrile affections, there is almost always a marked tendency towards increase of fever in the afternoon, succeeded by as obvious a tendency to its abatement during the following morning. In speaking of the condition of the pulse in *remittent fever*, Dr. Bolling remarks:—"Of course, in drawing our conclusions, allowance must be made for the increase in the frequency of the pulse, which occurs in *all febrile affections not strictly paroxysmal.* In our febrile diseases, *independent of the regular periodical exacerbations*, there is always a tendency to an *increase in the frequency of the pulse in the afternoon.*" (*a*) In his description of simple frank pneumonia, Wood distinctly states that "the fever *often* has a *remittent character; exacerbations* occurring *daily*, for the most part *towards evening.*"(*b*) In the pneumonia of infants and children at the breast, Bouchut directs special attention to the remittent character of the fever. He says: "The *continued type* is, as is known, *very rare; much more frequently the fever is remittent.*" (*c*) To such an extent is this sometimes the case, that on two occasions we have known pneumonia occurring in children (each one of which was about eighteen months of age), mistaken by experienced physicians of no mean skill, for attacks of intermittent fever, and it was not until after the disease had failed to yield to quinine, that a more careful examination led to the detection of its true character. We had supposed, indeed, that this law, so

(*a*) Amer. Jour. Med. Sci., N. S., Vol. XI, 297.
(*b*) Op. cit., Vol. II, 11.
(*c*) Pratical Treatise on Diseases of Children, 321.

nearly universal in its influence upon the pneumonia of febrile affections, would scarcely be questioned by any one; and surely there is abundant evidence that in simple frank pneumonia, occurring in localities where periodic fevers are never known to prevail, and where they are never seen, save in isolated instances imported from some malarious region, the febrile action very often presents a remittent and paroxysmal character, more or less marked. Dr. Manson, however, in his effort to draw the dividing lines between "malarial," and other varieties of pneumonia, expresses his conviction, that a "marked paroxysmal character is seldom or *never* observed in the simple form," and while he reluctantly grants on the averment of writers, "that in some cases of the latter form, the fever is sometimes evidently remittent," yet he "*strongly suspects the contrary.*"

We observe in the first place, that according to Dr. Manson's own admission, the febrile movement in the "malarial" is not "always distinctly paroxysmal and remittent," instead, he states positively, that "remissions are sometimes obscure, and active fever exists during the whole day and night." Well, if a paroxysmal and remittent character be the *type form* of fever in "malarial pneumonia," and yet sometimes be completely in abeyance, is there any reason why the *type form* of the fever in simple pneumonia should not likewise sometimes be lost, and that it may then assume "a marked paroxysmal" and remittent character? It cannot be denied, as it seems to us, that we often witness an evident remittent and paroxysmal character in diseases, that cannot by any possibility be referred to, or in any manner connected with, malaria, the essential cause of periodic fevers. It is well known that *paroxysms* of asthma occur generally at night, most frequently between bedtime and two and three in the morning, and continue to recur with *marked regularity about the same hours*, for nights in succession (*d*); that *hectic* fever is characterized by a strong tendency to a regular *paroxysmal* tpye, which sometimes approaches closely to intermittent fever; (*e*) and that

(*d*) Wood, Vol. I, 846; Chapman, Lec. on Dis. Thor. and Abdom. Vis., 130; Forbes, Cyc. Prac. Med., Vol. I, 207; Dickson, Op. Cit., 628-9; Barlow, Prac. Med., 481; Watson, Prac. Med., 717; Ellistson, Prac. Med., 768; Dunglison, Prac. Med. Vol. I, 331; Eberle, Prac. Med., Vol. 11, 208; Copland, Op. Cit., 179.

(*e*) Gerhard, Op. Cit., 179; Dunglison, Op. Cit., Vol. II., 537; Wood, Vol. I., 42.

in stridulous laryngitis or spasmodic croup, the *evening exacerbations* are well marked throughout the career of the disease. (*f*) It is well known that in inflammations affecting the sclerotica and iris, the pain is strikingly nocturnal, commencing after sunset, increasing in violence till after midnight, and abating towards sunrise, being scarcely felt during the day, but *returning about the same time in the evening.* (*g*) In children, febrile affections, no matter upon what cause dependent, almost invariably assume a decident remittent type, particularly in gastro-intestinal diseases. Every one must have noticed that in cholera infantum, the febrile movement is almost always of a remittent character, and attended by very marked *evening exacerbations.* The *remittent fever of children* is so remarkable for the distinctness of the periodical exacerbations and remissions of the fever, as to have obtained for the disorder the name that gives it so characteristic a title; and yet it will not be contended, surely, that the cause of this disease is identical with that which is productive of periodic fevers. In 1847, Rensward submitted to the Academy of Medicine of Paris, an account of several cases in which *periodic paroxysms of fever* were produced in a non-malarial locality by copious losses of blood; and in one of these instances the disease produced assumed a *regular tertian type*; in two others the fever was remittent. (*h*) An instance is recorded, in which a marble had been swallowed by a child in full health, violent symptoms of fever set in, almost immediately, assumed the *remittent form*, and lasted for a few days, until the source of irritation was removed, when they as suddenly disappeared. (*i*) A case fell under the observation of Dr. La Roche, in which six *febrile paroxysms* were caused by the ingestion of a peach-stone; and he refers to an instance from Copland's Dictionary of Practical Medicine, in which the same phenomena resulted from the formation in the stomach of a cheesy coagulum, and was relieved by its expulsion. Prof.

(*f*) Condie, Dis. of Children, 4th Ed., 324; J. F. Meigs, Dis. of Child., 2nd Ed., 70-71; West, Lec. Dis. Child., 217; Bouchut, Op. Cit., 282: Eberle, Dis. of Children, 346.

(*g*) Diseases of the Eye, by William Mackenzie, M. D., Hewson's Ed., 433; Lawrence, Diseases of the Eye, Hay's Ed., (1843) 325.

(*h*) La Roche, Op. Cit., 380.

(*i*) Practical Treatises on Diseases of Children, by Evanson and Mannsell, Phila. Ed., 243.

Dickson mentions a case of intermittent strabismus, in which the patient for many months, was subject to squint on alternate days, and he relates, also, the instance of an officer in the navy, who had been bitten by a centipede, the part injured undergoing some inflammation ; similar inflammation was renewed for years after, at regular intervals of six months. In some of the varieties of *herpes*, the eruption occasionally breaks out annually, about the same period, and for a succession of years.

Are all these instances in which the element of periodicity is so conspicuous, to be attributed to the same cause that gives origin to miasmatic fevers? Obviously not. Then we may conclude with Prof. Dickson, that periodicity itself is not so rare a circumstance in the history of diseases as to require that we should refer such as manifest it to the head of intermittent fevers on that ground exclusively.

"It is true," says this distinguished teacher, "that periodicity is a *very frequent element of disease in general*; nay, a certain degree of regularity of movement, of alternating or recurrent conditions may, perhaps, be regarded truly as the *rule*, and its *absence* the *exception*, in the *symptomatology of all morbid affections*." (*a*) Experience daily shows, remarks the learned and accurate La Roche, "that morbid irritations, by whatsoever causes produced, and in whatsover organic apparatus or system of the economy situate, may, and do assume the remittent and often do assume the remittent and intermittent types. These are observed in irritation of the red capillaries, inflammatory or hemorrhagic; in sub-inflammatory and nervous irritations. Of all these, examples may be found in works of easy access. In a word, there are *facts enough afloat, to prove that the element of periodicity does not belong exclusively to febrile diseases of a malarial origin*. So far from it, the *periodical* is as much a *natural* type as the continued. It characterizes many of the phenomena of health, and exhibits itself in the physiological play—both as regards progress and intensity—of many of the functions; in the process of secretion, elimination, and calorification ; in the operations of the nervous system ; in muscular contraction ; in the action of the heart, etc.

(*a*) Op. Cit., 76, 158-212.

Intermittence, indeed, may well be viewed as an element essential to the existence of the normal actions of economy." (*b*) Is there any reason, then, why this pervading law, so comprehensive in the scope of its ramifications, should not stamp its impress upon the vital phenomena in disease; and that periodicity may not be distinctly developed in the affections of many organs, without necessarily invoking the action of the subtle and inexplicable principle of malaria? If this intermittent and periodical law, impresses itself upon the obvious phenomena of diseases that are *known* not to be induced by malaria, why may not the febrile movement in simple frank pneumonia offer a marked remittent and paroxysmal character in a malarious region, independent of the action of that peculiar poison? We have not unfrequently seen cases of pneumonia during our residence in this section, in which the strictest scrutiny failed to elicit any positive and reliable evidence of a malarious complication, and yet, nevertheless, the fever was decidedly remittent and paroxysmal. During the month of March of the present year, we attended a sprightly young girl, ætat. 13, in one of the female schools of this village, who had a pneumonia of the summit of the left lung. Every morning there was a marked remission in the fever, the pulse fell 23 or 24 beats in the minute, became softer and less expanded, and the skin lost much of its febrile heat. Our special attention was given to this case, and it was watched with the most assiduous care by highly intelligent attendants. Had there been any evidence of a *chill*, unusual "coolness of the extreme parts," or a remarkable "reduction of temperature confined to the ends of the nose, toes and fingers," to which Dr. Manson directs our attention as being "almost invariably observed during the morning hours," and just before the rise of fever in his "malarial pneumonia"—such phenomena could not have escaped detection. Indeed, the case was watched with special reference to Dr. Manson's description of that form of the disease. This young girl did not come from a malarious region, and her mother informed us on her arrival, that her daughter had never suffered from any form of periodic fever in her life. She has certainly had noth-

(*b*) Op. Cit., 470.

ing of the kind since. The case yielded kindly to treatment in about a week, without the employment of quinine, or any other anti-periodic agent. We have attended similar cases with physicians of long experience in the diseases of this region of country, in which had their been any trustworthy evidence of a malarious implication, it defied our powers of perception. And he must, indeed, be an extremely careless or incompetent practitioner, who could over-look phenomena so prominent and self-imposing as those described by Dr. Manson as marking the "febrile action" of "malarial pneumonia."

2. We must aver, however, that we are unable to verify from our own experience, a part of Dr. Manson's description of the symptoms that accompany the daily paroxysmal and remittent course of the febrile movement, in this form of pneumonia. He assures us that the rise of fever is "*almost invariably*" preceded by "a *coolness* of the *extreme* parts;" the "ends of the nose, toes and fingers," and "may be confined to them;" that "this reduction of temperature" is not "*generally felt by the patient*," but "on the contrary, when it is present," that "*he* often complains *most intensely* of sensations of *excessive internal heat*, soliciting the introduction of cold air into the chamber, as Dr. M. has witnessed, and wishing the doors and windows to be opened, and the fires extinguished, when the thermometer stood below the freezing point near the bed of the sufferer." But we should not forget that during the morning hours of nearly all febrile affections, when there is abatement of the fever, the extreme parts not unfrequently present—and for obvious reasons—more distinctly marked than other portions of the body, the diminution of febrile heat. If this "reduction of temperature" is not duly considered, and proper allowance made for it, there is no little danger of mistaking it for that *coolness*, which Dr. Manson regards as indicating the presence of a "chill," and of thus greatly magnifying the number of cases of "malarial pneumoina." Trusting to such evidence as reliable tokens of the periodic fever element, it is easy to make "malarial," the "form of pneumonia generally seen by Southern observers," as well, we may add, as by the "rest of mankind." We are somewhat aided in the diagnosis, however, by the statement that, during the existence of "this coolness of

the skin," there are, "often," most distressing complaints of "*excessive internal heat.*" But an important inquiry suggests itself just here; are these phenomena usually present in that "phase of the disease," which is "*generally seen*" at the South? We think not. That they may have occurred in certain cases, there can be no doubt, since Dr. Manson has submitted the above sketch as the result of his own personal observations; yet, we cannot help thinking that he has adopted an incautious phraseology, and that the picture has been overdrawn. Let the experienced physician in the diseases of this region, attempt to recall to mind the number of cases of pneumonia that he has witnessed, in which during the forenoon of every day, and in the remission, or just before the rise of the fever, the patients "complained *most intensely of sensations of excessive internal heat*, and that too, in some instances, when water would soon freeze near their beds! We suspect that the number will be small—yea, very small. We had supposed that one "generally" feels *cold* when suffering from a *chill.* Of course, we are not ignorant that in certain forms of periodic fever—those usually termed malignant, pernicious, or congestive, and more espicially the *algid* variety—the patient may complain of heat, either internal or external, during the cold stage, although to the touch of others, his body may present even an icy coldness; but in the simple chill of ordinary intermittent and remittent fevers, the sensation of cold is often out of proportion to the actual loss of surface heat, and there may be heavy complaints of a feeling of severe cold, when, to the hands of bystanders there is abnormal heat.* We are persuaded that Dr. Manson must have generalized too hastily from insufficient data; and that phenomena he describes as "*often*" observed during the

* It may be proper to state here, that Dr. Manson alleges in his Essay on *remittent fever*, that the *chill* of *this* fever "*differs from that of any disease whatever!*" He affirms that the "*Initial* chill is commonly *felt* by the *patient*;" but the "*succeeding* chills differ in some important features." Thus, "the patient rarely complains of any *sensation* of *cold*, but often of *heat*, desires to be fanned, the clothing to be removed, and intreats for cold drinks." It is, perhaps, in consequence of his peculiar views in regard to the character of the *chills* of remittent fever, and their daily recurrence, as much as any thing else, that Dr. Manson is induced to declare in this production, "that there *cannot be found in any standard work a correct description of its characteristic features as it now presents itself!*" It has been now four years since these ideas were first set afloat, and they have been given more than usual prominency; yet, no one, so far as we can learn, has verified their accuracy.

period of chill, belong, on the contrary, to a few, and a special class of cases.

(*To be continued.*)

ON THE INFLUENCE OF THE TWO ORDERS OF NERVES WHICH PRODUCE VARIATIONS OF COLOR IN THE VENOUS BLOOD OF GLANDULAR ORGANS.

By Claude Bernard,

(Translated by Edward Warren, M. D.)

(I) The special chemical conditions, which cause the venous blood of glandular organs to assume a red tint at one time and and a black hue at another, are produced by two nerves distinct in their origin, and apparently dissimilar in their functions. That is to say, there exists not only a glandular nerve influencing the red blood circulation, but another which is instrumental in rendering venous blood black. Each of these nerves, in acting chemically upon the blood, modifies, but in an opposite manner the mechanical phenomena of the capillary circulation ; thus establishing a necessary correspondence between the chemical alterations produced by the blood in the tissues, and the physical conditions of the capillary circulation which are under the direct control of the nerves.

The experiments which have led to these results, were made upon the sub-maxillary of a dog, which is particularly suited to this investigation, because the intermittance of its secretion renders the variations of color very distinct.

The operation is, in fact, only a vivisection, and instead of being a dangerous and difficult undertaking, is so modified by the entire removal of the digastric muscle, as to become exceedingly simple. After this ablation, a deep wound remains, in the bottom of which the inferior surface of the sub-maxillary may be discovered, as well as the vascular and nervous organs whereon the proposed experiments may be easily made.

(II) The nerve, which causes the venous blood to appear red in the vein of the sub-maxillary, is a filament given off posteriorly to the lingual branch of the fifth pair, with which it is in contact simply, its true derivation being really from the seventh

pair, whilst it is principally made up of the corda-tympani. But however this may be, the glandular filament is easily reached at the point where it separates itself from the lingual to be distributed to the sub-maxillary, whose duct it accompanies.

Now, if the gland be examined, when it is duly provided with nerves, and in its state of normal quiescence, that is when nothing issues from its duct, the observer will perceive that its venous blood is decidedly black ; but if the glandular nerve, just described, be excited, the black blood of the vein is seen to grow brighter continually, until it becomes as rutilant as that of the arteries, if the irritation be sufficiently intense. This result is uniform, and establishes the physiological principle, that whenever the tympanico-lingual nerve is excited to energetic action, the venous blood of the sub-maxillary appears red, whilst, on the other hand, as soon as this nervous filament loses its excitation, wholly or in part, the blood returns again to its original color.

Nothing can be easier than to establish by positive proof, the special influence exerted by the typanico-lingual nerve, upon the red color of venous blood. In fact, when the glandular vein and nerve have been discovered, and an impression is made upon the sense of taste, by means of a little vinegar placed in the mouth, the blood becomes speedily red in the vein, because the gustative impression produced upon the tongue and carried to the nervous centres, is transmitted by reflex action through the corda-tympani.

The evidence of the correctness of this explanation is immediately supplied, for, if the nervous filament be cut where it leaves the lingual, the venous blood of the gland remains black, and from that instant, notwithstanding the application of vinegar to the tongue, and the impression made upon the sense of taste, the red tint does not appear, for the reason, that the medium through which the modifications of the blood are effected has been interrupted permanently. But again, if the filament be seized at the point operated upon, behind the lingual, and its cut extremity on the glandular side be irritated by means of galvanism, the effect of this artificial excitation is immediately apparent ; the blood becomes red in the vein, and then returns to its normal tint when the irritating agent has been removed. This last experiment

furnishes an additional argument in proof of the fact, that the red color of the glandular blood is always in exact correspondence with the activity of the tympanico-lingual nerve, and the black color of that fluid is in direct proportion to its physiological inactivity.

When the gland is in a condition of repose, the black color observable in the blood is due to the activity of another nerve which has that special function, and whose influence is both uniform in itself and antagonistic to the tympanico-lingual,—that being decidedly intermittent in its action.

(III) The nerve which renders the sub-maxillary blood black, originates in the great sympathetic, and is distributed to the gland, after following the two branches of the external carotid artery, of which the smaller penetrates the posterior and superior surface of the gland, whilst the other being the principal glandular artery, enters by the side of the excretory duct. The filaments of the sympathetic arise, for the most part, from the superior cervical ganglion, and anastomose with the branches coming from other sources, and particularly with the mylo-hyoiden, at the point where this nerve crosses the track of the facial artery. It has been before remarked, that when the sub-maxillary is observed in its natural state of repose, its venous blood is black, which is due to the fact that at that particular time the activity of the sympathetic exceeds that of thetympanico-lingual, which renders the circulating fluid red. This is easily proved, for if in this state all the sympathetic filaments are excised, the venous blood loses its black color and assumes a brighter hue, which becomes permanent, because the influence of the sympathetic is interrupted, and fails to reach the gland. But, if the activity of this nerve is artificially re-established, by means of galvanism applied to the glandular extremity, the black color returns, and continues until the irritating cause be removed. It may be asserted then, that the sub-maxillary venous blood is black when the sympathetic nerve acts upon the gland, and that the more potent this action, the deeper the tint presented. The variations of color are thus due to two nervous influences, which are well marked and distinct from each other. How shall this phenomenon be explained? There is no anatomical connection, and hence no immediate chemical action is possible on the part

of the nerves upon the globules of the blood by which their color can be modified. Certain intermediate conditions must be established between the action of the nerves and the resulting chemical changes in the blood, before the phenomena in question can be produced, and upon examination, these conditions will be found to consist in the mechanical changes effected by each nerve upon the capillary circulation of the gland.

(IV) The physical alterations of the capillary circulation established in the sub-maxillary by the tympanico-lingual and the great sympathetic, are exactly dissimilar. When the tympanico-lingual is excited, the venous blood becomes red, whilst the activity of the circulation increases until it bears a direct ratio to the change of color in the circulating fluid. A corresponding increase of the quantity of blood in the veins is observable also, as can be easily demonstrated. In one instance the blood leaving the glandular vein was measured, and it was found that during the quiescence of the organ, when the sanguinous fluid presented its normal color, it required sixty-five seconds to collect five cubic centimetres, whilst, when galvanism was applied to the tympanico-lingual and produced a flow of red blood, the same quantity was obtained in fifteen seconds, thus proving that the circulation in the last case was four times more rapid than in the former.

When the great sympathetic acts, the venous blood is invariably black, whilst the circulation is slower and feebler. A correspondence is likewise distinguishable between the rate at which the blood flows through the vein, and the intensity of its black color, showing the same law, but an opposite phenomenon from that noticed above. This sluggishness of movement is so distinct and exact that it may be carried to the point of an entire cessation, under the energetic action of the great sympathetic; but it will reappear, and become accelerated as before, whenever the tympanico-lingual is sufficiently stimulated.

The red and black color of the venous blood sustains a positive and uniform relation to the activity of the glandular circulation; but this activity cannot be produced directly by the nerves, for they have no immediate action on the blood, and it follows that the contraction and expansion of the vessels can alone explain these alterations in the properties of the circulating fluid.

(V) It can be easily established experimentally, that of the two nerves indicated, one dilates the vessels of the gland, and the other contracts them. The tympanico-lingual expands the capillary vessels, and the enlargement is so positive, when the nervous action is intense, that the blood passes from the artery to the vein without losing the cardiac impulse originally imparted to it in the general circulation ; and hence, it is seen to issue from the vein in interrupted jets, as if from a veritable artery. This pulsation ceases when the action of the tympanico-lingual abates partially or entirely. The sympathetic, on the contrary, contracts the vessels and lessens their calibre in the most decided manner. When this nerve is excited, the diminished vessels permit the passage of a constantly decreasing amount of blood, the sanguineous fluid is retained in the capillaries of the gland, and the circulation grows feebler, whilst the color of the globules darkens in a corresponding and parallel ratio. And, to sum up, the two nerves by which the color is modified, are motors, acting primarily by contracting or dilating the vessels. Of these, the sympathetic is the constrictor of the vessels, the tympanico-lingual is the expander of them.

(VI) In the physiological state of the sub-maxillary, that is, in its normal condition of quiescence, these two orders of nerves are equally active, and materially antagonistic to each other, so that the effective nervous power results invariably from the preponderance of one over the other, and to the special influence thus exerted at the moment, which influence does not affect the gland until the equalising force of the opposing nerve has been overcome. This is proved by the fact, that the action of each of these nerves is most intense and excited when its opponent has been so divided as to interfere with its normal function. This last phenomenon is very marked, especially in connection with the tympanico-lingual, for when this nerve remains intact, whilst all the filaments of the sympathetic are cut, and a few drops of vinegar are placed upon the tongue, the red blood flows through the vein with much greater intensity, and with more decided pulsations, than in the natural condition of nervous antagonism existing before the ablation. This difference of excitability in the tympanico-lingual corresponds with its normal physio-

logical excitant, the gustative impression, which shows conclusively that there exists a sort of functional equilibrium and counterpoise, resulting from the reciprocal antagonism of the dilator and constrictor nerves of the capillary vessels. The maximum dilatation of the capillaries corresponds with the direct passage of the red and pulsating blood; the minimum contraction of the vessels coincides with a languid circulation, and a dark color of the fluid; and between these extremes all possible intermediate states and gradations are discoverable.

(VII) To sum up, the two nerves only act in this connection as agents of dilatation and contraction of the sanguineous vessels. This influence, which differs in no material respect from that exerted by motor nerves in general upon contractile or muscular tissue, nevertheless produces a series of physico-chemical modifications in the blood, through the medium of a chain of phenomena perfectly natural in themselves. When the sympathetic nerve, which is the constrictor of the vessels, is active, the contact between the blood and the glandular tissue is prolonged, the chemical changes resulting therefrom are facilitated, and the blood circulating in the part presents a decided black color. When, on the other hand, the tympanico-lingual, which dilates the vessels, is excited to action, the blood passes rapidly through the gland, the modifications of venosity produced by the sanguineous and organic contact are accomplished in a manner essentially different, and the fluid issuing from the vein presents the color and appearance of arterial blood.

Thus, there may be always perceived an intermediate condition between the physiological action of the nerve and the chemical phenomena produced, which modifies mechanically the circulation peculiar to glandular organs. In fact, it is owing to the influence of these two nerves that the sub-maxillary really possesses a special circulation, which in its variations is independent of that of the general system. This special nervous system, which affects each capillary vessel and every organic tissue, regulates universally the entire circulation in its relation to the chemical states and functions peculiar to the various organs. These neuromic modifications of the capillary system take place without disturbing the neighboring organs or the general circulation. Each

individual organ is connected with the organism by those conditions common to the general circulation, and also by means of the nervous system; whilst each part may have a specific circulation and an appropriate physiology. Such are the special physiological states impressed upon the capillary circulation by the nervous system. There still remain for investigation the principles involved in the chemical modification of the blood, which takes place under these circumstances, whereby the changes of color in that fluid are produced. This will constitute the subject of another communication to the Academy.—*Archives Générales.*

CEREBRAL RHEUMATISM.

By Professor Lebert.

(Translated by Edward Warren, M. D.)

A man 22 years of age, who had suffered with articular rheumatism from his fifteenth year, was again attacked in December, 1856. Several of the articulations were successively invaded; the fever was intense and symptoms of pleurisy and pericarditis presented themselves at an early stage of the disease. After a temporary amelioration, all the symptoms became aggravated, accompanied by vertigo, and a miliary eruption on the skin. The swelling of the joints, the pain and the various other symptoms finally improved, though the pulse remained unchanged. On the 23d of December, the patient sank gradually into a comatose state—interrupted by occasional attacks of delirium and frenzy—the pulse increased in frequency, and death closed the scene twelve hours after the onset of the cerebral symptoms. The *autopsy* showed only a slight œdema of the arachnoid, and a little congestion about the meninges of the brain. Traces of the pleurisy and pericarditis, originally diagnosed, were discovered; the articulation of the right knee contained pus mixed with synovial fluid, but nothing was found of sufficient pathological gravity to explain the fatal termination. In this instance there was certainly developed a species of intoxication analagous to that of uremia, which view of the case is sustained by the fact, that the subject of this examination presented ecchymosis

of the gastric mucous membrane, and an enlargement of the isolated and conglomerate glands of the ileum—*Archives für Pathologische Anatomie.*

MEMORIAL TO THE LEGISLATURE.

Mr. Editor :

Permit me to call the attention of your readers to the memorial, submitted to the Legislature, on the subject of a Medical Board of Examiners for the State.

MEMORIAL OF THE MEDICAL SOCIETY OF THE STATE OF NORTH-CAROLINA.

To the Honorable, the General Assembly of the State of North-Carolina:

This Memorial of the Medical Society of North-Carolina, respectfully represents, that with a view to place the medical profession of the State on a higher level than it at present occupies, to organize it properly, to give it a collective existence, to engraft it on the State as one of its essential social institutions, and to furnish the citizens of the State with something of a guaranty, that their health and lives shall not be tampered with by unauthorized pretenders to medical skill; it is highly desirable that a Board of Medical Examiners should be appointed by your authority, to ascertain the qualifications of all persons who propose to assume the responsible duties of the medical profession.

In addressing themselves to so highly intelligent a body as the Legislature of the State, the Medical Society assume as a fact, not requiring proof, that a large portion of our population are entirely incompetent to judge of the qualifications of those to whom, by the force of circumstances, they are compelled to entrust their health and perhaps their lives, and if so, it clearly becomes the duty of the governors authority to furnish them such safegaurds against imposition, in this respect, as their better knowledge may enable them to afford.

In proposing to your honorable body the establishment of a Board of Medical Examiners, the Medical Society are influenced, in the first place, and chiefly, by their duty to society, and in the second place, by their duty to their own profession. Without assuming to themselves any greater measure of patriotism, philanthropy or loyalty to the State

than they are willing to accord to other enlightened citizens, they regard themselves as occupying a position which enables them the better to observe the wants of the community in this respect, and to determine the best mode of supplying those wants than others.

The results of their deliberations have been embodied in a bill which they desire to submit to your honorable body, with a confident reliance on your willingness to co-operate with them in any measure, having for its object the promotion of the welfare of the community, and of the reputation of the State.

The leading object of the measure proposed, is to guard that portion of the community which is obviously incompetent to judge of the qualifications of medical practitioners, against the impositions of impudent and unskilful pretenders, and in so doing, to elevate the standard of professional attainments among the legitimate disciples of the healing art.

Your Memorialists deem it necessary to advert to the fact, doubtless familiar to your honorable body, that in all governments, having any pretensions to a high grade of civilization, the medical profession is regarded as an important and necessary element of the aggregate social body or State. Apart from their more immediate and daily duties in the conservation and restoration of the health of the community, they are frequently associated with your Courts of Justice in criminal prosecutions, and in other important questions bearing on the dearest rights of citizens, in the adjudication of which, their testimony is of paramount importance. Without their aid, your courts would often be unable to hold the balance of justice evenly; without their aid, the shield which human laws extend for the protection of the insane, is liable to be interposed for the protection of guilt.

It is in the department of medical jurisprudence, that department in which the legal and medical professions coalesce, that the indispensible necessity of the medical profession as an integral and essential part of the social polity becomes prominently obvious.

The medical profession constitutes also a necessary and indispensable part of the military and naval establishments of all governments. And as all enlightened governments in modern times recognize it as incumbent on them to preserve a watchful care of the public health, and to guard their citizens as much as possible from pestilences, contagious and all preventable diseases, an intelligent and learned body of medical men furnishes, we respectfully submit, the only suitable

source from which sanitary commissioners should be selected.

It is in the performance of such public duties, as have just been referred to, that the words of the great poet of antiquity find their full verification:

> "A wise physician skill'd our wounds to heal,
> Is more than armies to the public weal."

Your memorialists regard it as among the proudest of our historical traditions, that the founders of our State, animated by a purity and single minded loftiness of patriotism—for which we can never be too grateful to their memories—and entertaining the most correct and enlightened views of social policy, did recognize it as their duty to society to make such provisions for public instruction as the pressing wants of an inchoate State imperatively demanded.

They correctly regarded the State as the legitimate and primal source of the authority granted to her literary institutions to confer degrees, and legalize the practice handed down from the colonial government, which required all those who undertook to expound the law in her Courts of Justice, to obtain a license for that purpose from the Judicatory of the State. Why the medical profession should have been ignored by the founders of the State, is a question which is perhaps incapable of a satisfactory solution at this late day. It may perhaps have arisen from the fact that at that time public attention, even in enlightened European governments, had not been particularly directed to it, or its importance recognised as a necessary part of the political fabric, or to the fact that other and more urgent cares were pressing on them—Assuredly, no good reason can be assigned why the legal, any more than the medical profession should derive its authority to practice from the State.

The recognition of the State as the fountain of honor, and the source of authority to practice law, has no doubt contributed to give an elevated tone and high character to that profession; and we may not unreasonably infer that a like result would follow from a similar public recognition of a connection between the State and the medical profession.

Such a recognition would give it vitality, would give it an existence as an institution of the State, and would contribute largely, we think, to its respectability and usefulness, and tend to elevate the character of the State.

The elevation of the character of the State must, we think, be regarded as one of the most important duties of her Legislature, and as the

State is composed of men of various professions and pursuits, none should be deprived of that countenance and fostering care which can be legitimately extended to them.

In advocating the establishment of a Board of Medical Examiners for the State, your memorialists are not proposing any novel or untried measure. On the contrary, it is one which has secured the approbation of the most enlightened members of the medical profession in other States, and also of the governing powers of those States, and the results have been such as may, and will, justify the State of North Carolina in following their example.

We hazard nothing in stating that in every instance in which the plan has been adopted, the grade of professional attainments has been elevated, and, in return, honor has been reflected on the States which have made it a part of their public polity.

The measure proposed will, we also think, if adopted, aid the medical profession in the establishment of a medical college, and thus enable them to throw off a galling dependence upon the institutions of other States for the only legal authority (if that can be so regarded) to practice their profession in the State. It will at any rate serve as a check to the suspected venality of some of the medical colleges of other States.

Should such an institution be established, it will aid in preparing the way for the organization of an enlarged system of public instruction; a system not limited, as at present, to our common schools and university, but one commensurate with the wants of the age, and which will enable our honored commonwealth to place herself fairly on a line with the most advanced States and governments of our day.

Claiming to be of the number of those who are truly solicitous to promote the dignity and honor of the State, we venture to indulge the pleasing anticipation that the day is not very distant when your Superintendent of Common Schools will be replaced by a Minister of Public Instruction, whose supervision will be co-extensive with the whole circle of the arts and sciences; a system truly polytechnic in its character, embracing in its wide sphere, not merely common schools and literary colleges, but law schools, medical schools, agricultural, military and engineering schools, &c.

And, as the Medical Society, in common with all enlightened patriots, are desirous of seeing our honored State, in all respects, a State complete—not lagging behind in the march of improvement, but progressing with a steady step to a high position among the enlightened

governments of our time—we respectfully pray your honorable body, as the legitimate custodians of the honor and dignity of the State, to take into favorable consideration the measure proposed, as one of the means of accomplishing so desirable a result.

JAMES H. DICKSON,
WM. G. HILL,
EDWARD WARREN,
S. S. SATCHWELL,
M. WHITEHEAD.

Committee.

CEREBRO-SPINAL MENINGITIS (ERYSIPELATOUS).

By J. J. Summerell, M. D., Salisbury, N. C.

An occasional case of cerebro-spinal meningitis I have now and then seen during my whole professional career (of nearly fifteen years;) but only during the spring of 1856, have I observed this disease to assume anything like an epidemic character. At that time however, during the months of March and April more especially, there was a great prevalence of what we had been accustomed to call *catarrhal fever*; a disease very closely resembling that usually described as such in the *books*. Many of the children and youth of the community were the subjects of its attack. The leading symptoms were drowsiness, heat of skin, rapid but rather feeble pulse, dry and slightly furred tongue, sordes on the teeth, much prostration of the muscular strength, great oppression in the nasal passages, which were dry and in most cases without any secretion whatever, and such a turgid condition of the schneiderian mucous membrane, as to render respiration through the nostrils painful and difficult.

When aroused the patient complained of most intense pain in the region of the frontal sinuses. There were always marked symptoms of catarrh, though by no means sufficient to account for the great amount of constitutional disturbance present. The bowels were generally confined, though sometimes unnaturally loose.

This condition of the patient would last for one, two, or even three days with little or no alteration, when very generally there was

observed a small red spot on some part of the face, most frequently on the eyelids and temporal spaces, slightly tumefied at first, but increasing rapidly, and in some instances becoming so great as to close the eyes in a few hours. In every case in which this swelling or eruption was well marked, there was a speedy amelioration of all the graver symptoms and convalescence dated from that occurrence. This manifestation of disease on the skin was unmistakably erysipelatous in its character, and gives a satisfactory clue to the nature of the epidemic. A *large proportion* of the cases terminated with a slight erysipelatous eruption on the face ; and as they every one, without exception, recovered, this event was hailed by the medical attendant as the sure ground upon which to base a favorable prognosis.

But it was not always our good fortune to behold this joyful harbinger ; for occasionally cases would occur in which there were no external evidences of erysipelas, but bronchitis or pneumonia would take the place of the eruption. And these cases under judicious treatment,as a general thing, did well. The treatment, however usually most successful in common sthenic thoracic inflammation was for the most part inadmissible here, and our chief reliance in their management was on the stimulating and supporting plan, modified according to circumstances. and combined with such mild mercurial impression as the asthenic character of the disease would permit with safety.

But there was another class of cases (and it is to these that the attention of the profession is particularly directed) which supervened upon the early symptoms described; and which from their fatal termination and the peculiar features they developed gave this epidemic its importance, and made it a subject of such deep interest to the physician where it prevailed. In these were developed all the leading symptoms of cerebro-spinal meningitis. The stupor gradually advanced to coma, or the pain over the brow increasing to the most excruciating agony, soon gave place to the wildest delirium. The pupils of the eyes in some cases became so permanently dilated as either to destroy or greatly impair the vision. In other cases they were closely contracted. There was more or less stiffness of the muscles of the neck and a disposition to opisthotonos was observed in one case. The poor victim soon

lost all consciousness and gradually sank as the functions of the brain and spinal marrow were more and more deranged or, as I saw once, general convulsions ended the scene. Such was the termination of some of the cases of this epidemic, a disease at its commencement precisely similar to the one we are accustomed to call "*catarrhal fever*," but widely differing in its sequalæ and results. It is needless to say that these brain cases almost always proved fatal. Cerebro-spinal meningitis occurring under the most favorable circumstances, is a very fatal disease, but as it presented itself in this epidemic not one well marked and well authenticated case recovered, out of some twenty cases occurring in the practice of the physicians of Salisbury. If there were any recoveries I did not hear of them. In my own practice there were none out of six cases.

Since the year 1856 erysipelas has prevailed more extensively in Rowan and Calanus than was ever known before, but cases of cerebro-spinal meningitis in any way connected with it, have seldom occurred. But whenever it has supervened on erysipelas it has been alike fatal, proving an opprobrium to the healing art.

At first, of course, this disease of the brain gave rise to much discussion among the members of the profession, and some diversity of opinion as to its nature. But as opportunities for observation were multiplied, and the epidemic more diligently studied and carefully analized, professional belief seemed to be unanimous, that these brain cases as well as those of bronchitis and pneumonia following these early symptoms of catarrh as described, were no less cases of erysipelas than were those where the eruption manifested itself on the face.

The question as to why this form of inflammation of the membranes of the brain and spinal marrow should be so much more fatal, than the ordinary epidemics of cerebro-spinal meningitis as they have prevailed in this country and Europe, which are described in the books, finds in my mind, a satisfactory answer in the nature of erysipelatous inflammation.

It is not only, as is generally believed, a contagious and specific, but also an asthenic inflammation or disease, and is always associated with depression of the vital functions and great vitia-

tion of the blood. Every practitioner's experience will attest, that *erysipelas* is more fatal than ordinary sthenici nflammation, even to the outer periphery of the body; for the pressure of the products of inflammation on important organs, or their confined condition does not here necessarily offer an insurmountable obstacle to the curative powers of nature. But when occurring in structures surrounded by unyielding bony walls,—themselves enclosing the brain, the very centre of life-giving power—it seems to me that it would be a very rare thing, even without a concurring experience, for a case of this disease to terminate in recovery. I have had no opportunities of making autopsies and therefore cannot speak with certainty of the *post mortem* appearances presented. But remembering the disease is erysipelas, occurring within the cranium, I think we may infer, that were such examinations made, we should find more diffuse inflammation of the membranes, greater tumefaction of the tissues, and a greater amount of serum effused than in sthenic inflammation, besides other products of an unorganizable nature, wholly unaccompanied by a curative adhesive process. Hence we argue the almost necessarily fatal character of the disease in spite of the best decided efforts in the way of treatment.

Before closing this notice it may not be improper to say something of the treatment pursued; and a few general remarks will suffice. When called to the cases of catarrhal fever as described, at an early period of the attack, an active purge of calomel and jalap was usually followed by some relief to the distinctive symptoms. The impression thus made we tried to keep up by an occasional small dose of the *submuriate*, and to moderate the force of the hearts action by small doses of emetic tartar, or in cases where there was irritability of the gastro-intestinal mucous membrane, ipecac. It was observed that as soon as the nasal secretion became free, the severe head symptoms abated in a marked degree. To assist in bringing this about, and to maintain it afterwards, a small flannel bag filled with fresh hops and steeped in hot water, was laid over the brow, and secured in its place by a napkin or cloth thickly folded to prevent a too rapid escape of warmth and moisture. Under such a course of treatment in the beginning, and quinine in the latter stages when the fever

was remittent, as it was apt to be, the patients soon recovered. When graver symptoms made their appearance, constituting pneumonia or bronchitis, the treatment, as before intimated, had to be conducted on the supporting and stimulant plan, rather than by depletions and depressions.

Where the membranes of the brain and spinal marrow became the seat of the disease, local or general depletion, in some cases which seemed to require it, calomel, mercurial incentives, blisters to different parts of the head and spine, and hyd. potassa, were the remedies usually tried, but were all alike unavailing. The patients died in almost every case, while the treatment seemed to exercise no influence whatever in postponing the fatal issue. The muriated tincture of iron, which seems to exert an almost specific power over erysipelas occurring in other portions of the body, I was detered from using, by a remark made by Prof. Barlow in relation to it in such cases. He says, (speaking of the treatment of erysipelas,) "in severe cases *where there is no head affection*, the *sesquichloride of iron* in doses of ten minims of the tincture three or four times a day appears to be almost a specific." But after some reflection on the subject, I am persauded that there is no sufficient reason for withholding the remedy where the encephalon is the seat of the attack, notwithstanding the warning of the Professor, because all other modes of treatment have effected next to nothing in staying its destructive power, and we know (and the Professor admits,) that this tincture has by the great success which has attended its administration in other forms of erysipelas, won the name of "almost a specific." At any rate if it should be my ill-fortune to encounter another such epidemic, or even a solitary case, I shall give the muriated tincture a fair trial, with the hope at least of better success than attended my ministrations in the spring of 1856.

And now Doctor, I close this imperfect account of erysipelatous cerebro-spinal meningitis with the regret that I did not particularly note the cases as they occurred in my practice. In the absence, however, of notes and even at this distant day, from the great interest excited in the minds of our profession in this section, I have thought that it might not prove uninteresting to

the readers of the Journal to have their thoughts directed to this subject, by giving the recollections of such points in the history of this disease as presented themselves to my mind, long after the scourge had passed away.

BIBLIOGRAPHICAL NOTICES.

THE INFLUENCE OF MATERIAL AGENTS IN DEVELOPING MAN.

AN ADDRESS DELIVERED BEFORE THE LITERARY SOCIETIES OF WAKE FOREST COLLEGE.

By S. S. Satchwell, A. M., M. D.

It always affords us pleasure to chronicle the success of a Carolinian; for the honor and interest of the "good old State" are particularly dear to our heart—the dearer because of the contumely which has been heaped upon her by those who, in the blindness of their prejudices, have mistaken her unobtrusive modesty for sloth or ignorance—and of course the more signal the success, the greater the satisfaction in recording it, and in offering our congratulations to him who has thus distinguished himself, and contributed towards a proper appreciation of the character of Carolina by the world at large.

Dr. Satchwell has been long known to the profession as one of our most accomplished physicians, and as an able and voluminous writer; but, in this instance, he has stepped beyond the exact limits of medicine, to gather fresh laurels in the more smiling fields of literature and science. With the proclivities of a true disciple of Æsculapius, he has, however, selected a subject intimately associated with those legitimately embraced in the professional curriculum, and which should engage the attention of every real medical philosopher. A consideration of the "Material Agents" that affect man's destiny leads directly to the question of the unity of the race, which is, indeed, one of the greatest import, since upon its final and satisfactory settlement, the truth or the falsity of the Bible must depend. Establish the fact that the account given by Moses of man's origin is false, and the whole doctrine of scriptural revelation falls to the ground, cov-

ering in its ruins all aspiration, hope and energy, closing the portals of Heaven alike against holy saint and hoary sinner, and leaving the world enshrouded in a pall of darkness and despair. If incorrect in this great fundamental proposition, the whole record is unworthy of respect and confidence. If the curse and the increase came not from Adam, the whole story is a shameless fabrication, and faith becomes the most complete and unpardonable foolishness. But on the other hand, if the researches of science establish the accuracy of scriptural ethnography; if the multitudinous phenomena of philological, physical, intellectual and moral diversities presented by the history of the race can be explained away; if the conflicting peculiarities of particular sections and circumstances can be harmonized, then, the truth of the Bible in this respect is confirmed, the "glorious plan" of man's salvation is made a living and blessed reality, and the hope of an immortality beyond the grave cheers the heart with promises of a higher and nobler destiny.

The first intellects of this, as of previous ages, have devoted themselves to the elucidation of this most momentous question. Human ingenuity has taxed itself to the utmost limit of its capacity in the solution of this great problem. History, Geology Anatomy, Philology and Psycology have all been appealed to, in the eager search for arguments, by the sturdy champions who have contested this hard-fought field. But in our judgment all the victories (?) which infidelity has ever won, have been mere intellectual triumphs—sophistical trophies—such advantages are only "worse than a defeat," and demonstrate the intrinsic weakness of the cause of skepticism, as well as the latent power of religion and the Bible. The lists are still open, and he who comes forward to do his *dévoir* in the cause of the Bible, and in support of its teachings, is a hero up to the full measure of his own sincerity, and a benefactor to the race in proportion to his success in the contest. The main point in Dr. Satchwell's Address, is that in which he attempts to prove that "Physical Causes" have produced the distinct differences which manifest themselves between the various branches of the human family, and that their existence is no evidence of other than a common origin for the races of the earth. Without going into details,

by a few bold and decided touches, he brings out the whole subject, clothes it in appropriate colors, and so photographs it upon the minds of his readers as to preclude the possibility of a false impression for the future, whatever the light or shade that sophistry may throw upon it.

But this is one only of the multitudinous relations in which the subject of "material influence" is considered. It is succinctly traced out and accurately delineated in all of its various ramifications. Its connection both with the physical and material world is illustrated in a masterly and impressive manner. The wonderful agency that this great principle has exerted in moulding the intellectual character of individuals and of nations; in developing the moral nature of the human family; in promoting the advance of arms, arts and letters; and in giving to particular races dominion over the earth is made most clear and manifest. And, in a word, the subject is as fully and thoroughly discussed in this little pamphlet of twenty pages, as many others of more extended reputation could have done it in a folio.

TYPHUS FEVER IN GREAT BRITAIN.

By I. B. Upham, M. D.

This is an interesting pamphlet of 46 pages, containing a concise account of typhus fever as it has occurred in Great Britain, and many cases illustrative of the pathological character of the affection, together with important observations respecting the treatment of the disease. We have read the little work with pleasure, and can not now conclude our notice of it without quoting the following observation of Dr. Jenner, which has more than usual significance in these days of "self limitation," "rational medicine," and donothingness, as taught by Sir John Forbes and others of his peculiar school.

"In no disease is the prompt use of powerful remedies more clearly indicated than in typhus fever. It is in determining when to act, and when to do nothing, that the skill of the physician as a curer of disease, or rather with reference to fever as an averter of death, is shown. Interfere, bleed, or stimulate, when nothing should be done, and the patient is lost. Refrain

from depletion or withold wine, when the one or the other is required, and the patient sinks into that grave from which a judicious treatment might have saved him."

LECTURES ON THE PRINCIPLES AND PRACTICE OF PHYSIC, DELIVERED AT KING'S COLLEGE LONDON.

By Thomas Watson, M. D. etc.

A new American, from the last revised and enlarged English edition, with additions by Francis Condie, M. D., one hundred and eighty-five illustrations on wood. Philadelphia, Blanchard & Lea, 1858.

Through the kindness of the publishers, this admirable work has been placed upon our table, and we can but express our admiration, of the excellent manner in which this edition has been gotten up, and presented to the public. It is an honor to American typography, as well as a most convincing proof of the skill, taste and enterprise of those by whom it has been prepared for the profession. With the lectures of Dr. Watson every medical man is familar, for they have had a circulation wherever the English language is spoken, as extensive as that of any other work of the present times. They are characterized by a simplicity of style, and aptness of illustration, an elegance of diction, which, independent of their intrinsic merit as scientific productions, have won for them universal admiration. A certain manliness distinguishes them that is exceedingly attractive and interesting. The feelings of the lecturer seem no less interested in his class than the subject; and the reader realizes continually, that he is dealing with a man, as well as with a devotee to science. And beyond all this, there is such a strong substratum of common sense in his medical philosophy, such plain and comprehensible expositions are given of pathological principles, and such simple, reasonable, and logical rules are promulgated for the treatment of disease, that the book cannot fail to recommend itself to every practitioner of medicine. Dr. Watson has revised the work himself, correcting whatever his own experience, and the observations of others, have proved incorrect,

and adding to it all the new facts which have been evoled by the progress of medical science. Dr. Condie of Philadelphia, who should be christened *the indefatigable*, is the editor of the present edition, and has performed the task with his usual industry and success. Numerous notes have been added by him, which in themselves are valuable contributions to medical science, as well as ornaments and improvements to the book. This work is an admirable text book, and should be in the hands of all engaged in the study and practise of Physic.

A SYSTEM OF HUMAN ANATOMY, GENERAL AND SPECIAL.

By Erasmus Wilson. F. R. S., Author of "Dissectors Manual, A Treatise on Diseases of the Skin, &c. A new and improved American, from an enlarged London edition with three hundred and ninety-seven illustrations. Edited by Wm. H. Gobrech, M.D., Professor of Anatomy in the Philadelphia College of Medicine, &c. Philadelphia, Blanchard & Lea. 1858.

This work has so long held a prominent place in the estimation of the Profession, and has received such a thorough practical test, as a text-book, in the hands of medical students, that it is unnecessary for us to speak anything in its commendation, in presenting for notice the beautiful edition, just published by Messrs. Blanchard & Lea.

The author seems to have been stimulated by the favor with which the work has heretofore been received, to unusual care and labor in the preparation of the present edition, and has made numerous important additions to the work, in all of its departments. It is now, without doubt, the most complete and thorough treatise on Human Anatomy, in the language, as well as convenient and reliable book that can be placed in the hands of students. The work is extensively and accurately illustrated, and in every particular speaks well for the industry and attainment of Dr. Gobrecht, its accomplished editor.

BRIEF EXPOSITIONS OF RATIONAL MEDICINE.

To which is prefixed "The Paradise of Doctors:" a Fable. By Jacob Bigelow, M. D., late President of the Massachusetts Medical Society, &c. Boston—Phillips, Sampson & Co., 1858.

We have read this book with little pleasure and less profit. *Rational Medicine* has become a sort of watchword, by which the enemies of the Regular Profession have been rallied into an organized opposition against principles that we honor and reverence—not as mere relics of an unphilosophical age, or the offspring of vague and illogical speculators alone, but as legitimate deductions from facts which the accumulated experience of centuries has eliminated and established. This, a dilution, and a very weak one, of Sir John Forbe's work on the same subject, which at least has merit of originality—however false its doctrines and incorrect its conclusions; and to our taste, it is as uninviting a dose as the profession was ever called upon to swallow. The main proposition of this work is, that all medication save that inculcated by this "new school," is absurd in itself and dangerous to the patient; which of course convicts all those upon whom this marvellous revelation has not fallen, of stultification and criminality at every step of their professional career. There is a degree of assumption in all this, which is both ridiculous and contemptible. The vanity and conceit thus exhibited, are as amusing as they are characteristic of Dr. Bigelow's particular locality. This indirect attack upon those who prefer to follow the light of experience and common sense rather than the delusive guidance of mere speculation and theory, is unworthy of any man holding a position of influence in the profession. We regret to observe, that many of the Journals have spoken approvingly of this work, and that its principles are warmly advocated in some quarters. As for ourself, we have no particular respect for the locality from which this book has emanated; for there is an atmosphere of exclusiveness and presumption pervading it which is extremely offensive; there is a degree of affectation, cant and superciliousness about it which disgusts us heartily; a haughty, dictatorial, and overbearing spirit characterizes it; such as originates only in that most disagreeable assumption of superiority, which is always contemptible; a certain patronising con-

decension distinguishes the dealings of its people with the outside barbarians of other latitudes, which indicates a firm conviction that eternal gratitude should be the reward of their slightest notice. And there is many a man living in those illuminated regions, who honestly believes that all the wit, wisdom and learning of the world, has been concentrated in Boston by a special dispensation of Providence, and that the glory of Athens and Rome was as insignificant, when contrasted with that of their wonderful city, as "the flicker of a farthing candle to the blaze of the meridian sun." On this account, we are very far from following the prevailing fashion of conceding to the profession there all that it claims for itself.

We can find no pleasure in ministering to the vanity of men who are already inflated by it, far beyond the capacity of ordinary mortals. We cannot follow them in all the transcendental refinement with which they delight to confound themselves, and to startle the country. We can but view this attempt to promulgate the pernicious doctrines of Forbes and Bennett, with anxiety and alarm, when it originates with those whose lofty pretensions have given them so much influence everywhere; and we take this occasion to deprecate the movement, to protest against the principles of this new philosophy as destructive to the best interests of medicine, and to warn the profession against this effort to prostrate and paralyze it, by those who should be its champions.

By Rational Medicine, in its proper meaning, we understand simply, *common sense* in the practice of physic; prescribing according to the indications, relying upon no exclusive system in the treatment of disease, but invoking all reasonable plans, whether heroic, expectant or exclusive, to the aid of nature in her struggles for relief, as they may be demanded by the exigencies of the case. We shall pursue this subject at another time.

PRACTICE.

ON QUINOIDINE IN THE TREATMENT OF INTERMITTENT FEVER.

By J. DaCosta, M. D.

Amongst the preparations of bark that have occasionally been used for the cure of intermittent fever, we find quinoidine, or the so-called "amorphous quinia" mentioned.* Yet, although the price of this article is considerably less than that of the favored salts of its twin-alkaloid, quinia, it has met with but little medical notice or employ; the publication, therefore, of upwards of fifty cases treated successfully with quinoidine, may prove, the writer hopes, not uninteresting nor valueless to some of the readers of this journal.

Chinoidinia, or "quinoidine," was first brought into notice by the supposed discovery of Sertürner of a new alkaloid, obtained by treating acidulous extracts of cinchona bark by alkalies. This alkaloid he found associated with quinia and cinchonia, and he procured it in large quantities from the resinous substance remaining after the crystallization of sulphate of quinia, which mass itself had previously been employed as an antiperiodic in this city, under the name of "extract of quinine." (See U. S. Dispensatory, 10th edition, p. 1171.)

Several chemists, induced by Sertürner's description of the new alkaloid, endeavored to procure it in its isolated state, as he stated he had done, but they were not successful; and the opinion, strengthened by the experiments of MM. Delondre, Henry and Geiger, soon gained ground, that the supposed new alkaloid discovered in the mother liquor after the crystallization of sulphate of quinia, was in reality quinia and cinchonia, in union with a resinous substance, which impeded their crystallization. Liebig,† who analized several samples of quinoidine, found a considerable proportion of it to possess the chemical characters of a true organic base, and to have the same atomic weight and composition as ordinary quinine, to which he conceived it to bear the same relation that uncrystalline sugar does to crystalline.

* To guard against error, it may be well to state that quinoidine is an entirely different substance from the quinidine, the sulphate of which has been recently discovered to possess antiperiodic properties. (See Med. Examiner, Vol. x.)

† London Lancet, May 23, 1846.

The resinous alkaloid obtained by evaporating an ethereal solution of the flakes, (precipitated from an acid solution by ammonia, and alkaline carbonates,) he termed, therefore, "amorphous quinia," and this veiw of the nature of quinoidine seemed to be confirmed by the statement of Winkler, that ordinary quinia, may be rendered amorphous by the action of acids, and by the experiments of Roder, (Chem. Gaz., 1848), who obtained from the amorphous quinia, quinia in a crystalline state. Shortly after the publication of Liebig's analysis, a patent, probably through his influence, was granted to Mr. Bullock, of London, for the manufacture of the "amorphous quinia," or this new extract from the quinoidine. The process, however, by which the latter was thus entirely deprived of the resin, and reduced to "amorphous quinia," rendered this an extremely expensive preparation, nor was it found to differ from the ordinary quinoidine of commerce, except in its being more highly purified.* Later analysis than those of Liebig have tended to confirm the fact that quinoidine is in reality the amorphous alkaloid quinia, in union with a resinous substance, and capable of forming salts and acids; yet there are some chemists who consider it to be a compound of quinia, cinchona, and quinidia, and others again who still regard its true composition as unascertained.†

Quinoidine sufficiently pure for medicinal purposes, is prepared in this city by several eminent chemists, (especially, we believe, by Messrs. Powers & Weightman,) by precipitation, by means of alkalies, from the acidulous solution of the residue left after the crystallization of the sulphate of quinia. The precipitate thus formed is dissolved in alcohol, and from this solution, subsequently, the quinoidine is obtained. The amount of this alkaloid furnished by different barks varies. Thus, the red bark, which contains less quinia than the Calisaya, yields a larger amount of quinoidine. Mr. Weightman recently informed the writer that the ordinary Calisaya bark furnishes, in proportion to every 100 parts of quinia, only about 10 to 20 of quinoidine, while the New Granada barks yield in proportion to the quinine they contain, about 40 per cent. of the quinoidine.

* See Mr. Redwood's paper, Pharmaceutical Journal, Vol. vi, p. 131.
† Ibid, Vol. xi, 1850

Quinoidine, when pure, is soluble in alcohol and sulphuric acid, from which it may again be precipitated without loss of weight. Its color is that of an ordinary extract; it is almost entirely devoid of taste. It has been occasionally medicinally employed, especially in some parts of Continental Europe, and enjoyed a high reputation as an antiperiodic in an epidemic that occurred near Berlin, (see London Lancet, 1846,) so much so, that the peasants came from fifty miles distant to procure certain "fever drops," which consisted of the medicine in question, dissolved in sulphuric acid. In this country quinoidine, owing, perhaps, to its liability to be adulterated, has been but little employed, excepting by the venders of quack medicines, whose nostrums in many instances, owe their efficacy to the quinoidine they contain.

The therapeutical effect of quinoidine, as far as the writer has observed them, are very nearly those of the ordinary sulphate of quinia. In doses two thirds larger than those of the latter, he succeeded perfectly in checking intermittent fever; but it was never noted to give rise to the headache, nor the ringing and buzzing in the ears, nor to the sickness of the stomach, which so frequently attend the adminstration of the sulphate of quinia, or of the other ordinary preparations of bark. It was sometimes given in doses of 40 grains, without the slightest inconvenience to the patient resulting from it, and in no case did it fail in checking the periodical returns of the paroxysms. In a few instances, indeed, it proved successful when sulphate of quinia had been previously administered without result. As a tonic, it seems to possess the same advantages as any of the other preparations of bark. Age does not constitute an objection to its use, for it was employed with equal advantage in the very young and the very old.*

The patients for whom quinoidine was prescribed, were mostly Irish of the lower classes, who applied for advice to the Moya-

* In proof of the efficay of this medicine, the writer is happy to be able to add the testimony of one of the gentlemen connected with the chemical works of Messrs. Powers & Weightman on the falls of the Schuylkill. He informed him, that in all the cases of intermittent fever which occur there among the workmen and their families, quinoidine had been used with perfect success, and that the effect was always permanent, which could be easily ascertained, as most of them were under his daily inspection. The medicine was even deemed more efficacious than the other preparations of bark.

mensing House of Industry. In many, the intermittent fever had lasted for some length of time ; it was mostly of the quotidian type. No cathartic was ordered previous to the administration of the medicine, which was given in pills of two grains each, or else dissolved in dilute aromatic sulphuric acid. The average dose required to arrest the chill in an adult seemed to be about 20 grains, six grains of which were, as a general rule, ordered shortly before the the expected paroxysm, whilst the rest was taken during the intermission. If the bowels were constipated, a little of the extract of colycinth was with advantage joined to the quinoidine. After the chills were checked, the remedy was in some cases continued, in smaller doses; yet as patients, when relieved, are not always willing to return to a public institution, it was sometimes impossible to persevere with the treatment, and the disease then generally reappeared after the lapse of a week or two.

Of the 53 cases treated, in many of whom the disease was of long standing, the chills were arrested in 49 cases by the first administration of the medicine ; only 4 required a repetition of the dose. In 10 cases the disease returned, which, although it may seem a large proportion of relapses, is not in reality so, when we consider the well known tendency of intermittent fever to return, and the fact that, in many of the cases, no medicine, for reasons above stated, was given after the first arrest of the chill. In conclusion, the writer can state as his honest belief, that quinoidine possesses antiperiodic qualities which, if not superior, are certainly not inferior to the sulphate of quinia or cinchonia, whilst he thinks it preferable to these, from the absence of bitter taste, from its being less liable to affect the head or the stomach of the patient, and from its comparatively low price.*

QUININE IN SCARLET FEVER.

Dr. E. A. Morrison, of Lawrenceville, Va., has an article in the *Virginia Medical Journal*, in which he extols this new method of treatment. He has treated this disease repeatedly in epide-

* About one-seventh of the price of the sulphate of quinia.

mic form, for more than thirty five years, and thinks he has met with the greatest success from the powers of quinine. He relates several cases where it had the happiest effects, though the disease presented itself in a very malignant form.

He treated about twenty cases in one family, and all recovered but one, to which he was called at the last moment. When the first symptoms make their appearance, he orders quinine, regulating the dose according to the age, and continuing it every two or three hours, until the patient is under its influence; occasionally giving a little mild cathartic medicine, to gently relieve the bowels, and mopping the throat with a strong solution of the nitrate of silver. He also, when the patient is old enough, directs a gargle of red pepper tea and common salt.—*Med. and Surg. Reporter.*

PUSTULA MALIGNA.

Dr. Rafeal, of Provins, has successfully treated five cases of pustula maligna by the application of fresh walnut leaves. Nelaton reports relative to them (*Archiv. Gen.*, Nov. 1857), that R. is a scientific and trustworthy observer: and his results only confirm observations made elsewhere, particularly by Pomayrol in the south of France, who had successfully treated forty cases in the same manner. On the other hand, Bourgeois, in the *Gaz. hebdom.* (1857, 41–42), remarks that an ample experience (more than 700 cases!) had convinced him that pustula maligna, like other diseases, had its mild and its virulent type—in general, a dangerous disease; it sometimes, as last summer, ran its course more favorably without the use of other than emollient and discutient applications. fit rivals in efficacy to walnut leaves! B. regards malignant pustule as the immediate effect of the direct inoculation of organic matter from animals (ruminants, solidipedous, rodents) affected with milzbrand (a typhoid disease). Timely destruction of the affected part is undoubtedly the best treatment; and B. prefers the caustic potass to the actual cautery, as being less fearful and quite as efficacious. The part affected must be completely destroyed, and removed by a piece of linen, and the cauterization continued till blood appear. If timely used,

this method is almost certain to be followed by a fortunate result.—(*Prager Vtljschft.*, 1858, 2*ter Band.*)

NEW PHYSICAL SIGNS.

At a late meeting of the New York Academy of Medicine, Dr. Corson, in a paper on the management of the arms and shoulders in chest examinations, announced two new physical signs in the diagnosis of thoracic diseases. The first was called *moist respiration*, and was compared to breathing through a layer of wet sponge. It is heard before and after the mucous râles of bronchitis. This sound can be heard after causing the patient to separate the scapulæ, and to render tense the muscles of the back by crossing the arms in front, and grasping and pulling forward the shoulders strongly with the hands, while the physician pushes down the shoulder blades. Even in health, according to Dr. Corson, the respiratory sound can be nearly doubled in this way.

The other sign was the comparative *stiffness* in the motion of the shoulder over the lung most diseased. This is observed by carefully watching the play of the inferior angles of the scapulæ during full expiration. The deficiency of motion may be mainly either "acromial" or "angular," the difference seeming to depend on the higher or lower situation of the disease which thus paralyzes the parts nearest. This stiffness of the shoulder was seen least in recent attacks. It varies most in different stages of phthisis, is slightest in pneumonia, and greatest in pleuritic affections. The value of these signs will doubtless be tested by intelligent observers.—*Boston Med. Jour.*

TREATMENT OF ASTHMA.

One of the most valuable remedies that recent experience has furnished, consists in the inhalation of the fumes of burning nitre paper. How or by whom it was discovered, or where, I know not; but from the references made to it by different authors, it must have been in use for nearly twenty years, and its great value and efficacy are now beyond question.

I might multiply cases to almost any extent; hardly a week passes without my meeting one or more. Within the past week

I have met with two, in one of which the nitre paper was the only thing that did any good, in the other its efficacy was shared by stramonium. It has been suggested to me that if the nitre is dissolved in a strong infusion of stramonium instead of water, the result is more satisfactory, but of that I have not made trial.

Let me give a few practical hints with regard to the making of the nitre paper. And this is not an unimportant point, for patients will find it more convenient to prepare the paper themselves, and if it is not properly made, it will not produce its beneficial results. The object is to have as much deflagration of nitre and as little combustion of paper as possible. For that purpose the paper must not be very thin, or it will not take up sufficient nitre; or very thick, or it will make the fumes too carbonaceous; but it must be moderately thick, and very porous and loose in its texture, so as to imbibe a sufficiency of the solution. The strength of the solution should be saturate at the ordinary temperature. If a saturate solution is made with warm water, and the paper is very bibulous, it becomes too much impregnated with nitre—too strong a paper, and burns too fast, with a sudden explosive flame. There should be no brown smoke in its combustion, but light, clear, white fumes. Those who have had a good deal of experience with this remedy tell me that they find the red blotting paper, of moderate substance, the best. Some blotting or filtering papers appear to have a good deal of wool in them; they are loose, thick and coarse. They should be particularly avoided, as they yield, on burning, a smoke of a particularly irritating and offensive kind, something like the smell of brown paper smoke, only worse. The nitre paper, when once made, should be kept in a dry place, and then will not be the worse for any amount of keeping; but if it gets damp, it does not burn with sufficient freedom, and should then be dried before using.

The following is the way in which an asthmatic gentleman tells me he has been accustomed to make a paper that answers perfectly well:

"Dissolve four ounces of saltpetre in half a pint of boiling water; pour the liquor into a small waiter, just wide enough to take the paper, then drain it through the liquor and dry it by

the fire; cut it into pieces about four inches square, and burn one piece in the bed-room on retiring to rest at bed time." I have tried this method of preparing the paper myself, and find that it burns perfectly well and is very efficacious; but I think two pieces are not at all too much to burn at once—*Virginia Med. Jour.*

SURGERY.

ON DISLOCATIONS OF THE HIP-JOINT.

Roser (*Archiv. fur Phys. Hkde.*, 1857, 1 and 2 Hft,) adduces in respect to dislocations of the hip. the following experimental maxims: 1. The true obstacle to reduction of a dislocation is almost always the smallness of the tear in the capsule, and not the tension of the muscles. 2. The difficulty in reduction consists in finding the direction and position in which the dislocated head of the femur corresponds to the hole in the capsule, these being found, the slightest impulse or rotation, suffice to complete the reduction. 3. As much depends on the situation and direction, as on the size of the rupture of the capsule. 4. The dislocated head does not generally preserve the original position in which the luxating force places it, but by an immediately following flexion, adduction, etc., takes up a secondary position. 5. All attempts at reduction must, therefore, be commenced by restoring the dislocated bone to its primitive position, and causing it to glide from that into its normal situation. 6. The dislocations of any given joint must therefore be arranged according to the essential anatomico-mechanical causes. The symptomatical arrangement of Malgaigne is alike unscientific and unpractical. 7. The most common dislocation of the hip is downwards, the head of the femur coming out beneath the tendon of the obturatorious internus. 8. This dislocation is caused by a strong and forcible flexion of the hip-joint during a slight rotation of the thigh inwards. 9. The dislocation downwards is commonly changed by a movement of extension (together with rotation inwards) into the dislocation backwards, luxatio ischiadica. 10. It can be changed by adduction into the luxatio obturatoria. 11. In the reduction of all such luxations, in which the tear in the

capsule is to be found below, strong flexion seems first necessary to bring the head of the femur into its primitive position opposite the tear. 12. The dislocation into the foramen ovale seems a mere variation of the dislocation downwards. 13. In the luxatio obturatoria, the head of the femur lies under and behind the obturator externus. 14. Malgaigne's luxation perinéale is only a slight modification of the luxatio obturatoria. 15. Over-extension of the thigh, with rotation outwards, must be regarded as the cause of the dislocation forwards between the fossa ileopectinea and the psoas. A secondary displacement is also caused by a slight movement of flexion. Reduction is produced by restoring the thigh to its over-extended position, and rotating inwards. 16. The dislocation behind the pectinæus must be regarded as a modification of this luxatio ileopectinea, and must not be looked on as belonging to the luxatio obturatoria. 17. The very rare dislocation outwards towards the anterior and inferior spine seems a modification of the luxatio iliaca brought about by forcible rotation outwards. 18. The luxatio iliaca is in truth the rarest of all dislocations of the hip, though usually regarded as the most frequent. Most of those usually reckoned as such, and even a good many of the cases to which Malgaigne gives this name belong to the luxatio ischiadica. 19. The luxatio iliaca is essentially caused by rotation inwards during adduction and flexion of the joint. Reduction must be based upon rotation outwards. (These maxims are illustrated by striking examples; and R. deserves great praise for his attempt to reduce the dislocation of the hip, so minutely subdivided by the great French surgeon, to simpler and more physiologically correct forms.) By spontaneous dislocation of the hip, Roser understands (*Froriep's Notizen*, 1857, 17,) not such as take place after destruction of the joint, by the mere weight of the limb, by muscular action or any external cause, sufficient to move the head from its socket, but such only as pre-suppose an elongation, extension, or relaxation of the capsule of the joint. It is conceivable that so soon as the capsular and round ligaments are considerably elongated, a dislocation of the hip may occur quite spontaneously, *i.e.*, without the application of external force, from the mere motion of the sick person in bed, or from a simple rotation of the thigh.

The occurrence of such dislocations has been observed by Brodie, Stanley, Stromeyer, Parise, Bonnet, Malgaigne, and R. himself. Watery effusion into the capsule has been regarded as the cause of the elongation, and even as the cause of the dislocation, but although effusion filling the capsule may cause it, in assuming a more sphærical form, to extend downwards and drag with it the head of the femur, as Parise saw in his forced injections of the capsule, yet effusion is not necessary for the production of the dislocation, and may even be a sequence and not a cause thereof; and whenever the capsule is anywise elongated, dislocation is to be dreaded, and the diagnosis may be settled *per viam exclusionis*. Spontaneous luxation as a consequence of acute (pyæmic) suppuration is caused by destruction of the capsule, as a consequence of chronic scrofulous suppuration, chiefly by destruction of the bones, while in cases preceded only by rheumatic inflammation, by contusion with extravasation of blood, or where it has suddenly happened unpreceded by suppuration, we must refer it to relaxation of the capsule; this is particularly the case in cases of spontaneous luxation in typhus, as was suspected by Schotten, and proved by the direct observations of Loriuser, Helwig, and Stromeyer. There are also cases of paralysis of the surrounding muscles, in which the dangling limb becomes spontaneously luxated, and here also effusion into the capsule may be the efficient cause, inasmuch as Stanley's patient had been paralytic for twenty-two years before his joint was luxated. Similar cases are undoubtedly rare, and form no proper objects of treatment. Spontaneous luxation, chiefly imperfect, also occur through wearing down of the posterior and superior edge of the acetabulum; and, in old cases, it may be difficult to distinguish between this form and that produced by relaxation of the capsule; however this form occurs slowly, is accompanied by great immobility of the head of the femur, and is incapable of reduction. The *diagnosis* of dislocation of the hip is established: 1. By measurement, during which both extremities must be placed in the same position, in order to avoid mistakes caused by obliquity of the pelvis. Error may, however, creep in, caused by deficient development of the thigh bones from previous hip disease, or absorption of the acetabulum, or head of the femur. 2. By feeling

the head of the femur in its anormal position. Mistakes may also happen in this, particularly when the dislocation is uncombined with morbid rotation or adduction, or if great swelling exist. The abducted and outwardly rotated limb may sometimes be improved by pressing the trochanter inwards by an abduction splint laid over a cushion fastened above to the pelvis, and below to the thigh above the knee; the rotation outwards may be kept up by keeping the toes out with a bandage or towel fastened to a splint, the mattress, or side of the bed; should extension be also desired, the weights may also be applied, the pulley being placed in that corner of the bed corresponding to abducted position of the limb; these simple appliances thus accomplishing all that the complicated machines of the orthopœdists are supposed necessary to accomplish. Only after months can such patients be allowed gradually to attempt walking; adduction being still promoted by a girdle round the hip-joint, and rotation inwards being carefully guarded against. Should reduction be from any cause impossible, the position and mobility of the femur may still be improved, and the patient made to limp less, or even enabled to give up his crutch. Scars and traces of fistulæ form no contra-indication to efforts at improvement, provided the constitution be sound and mobility present. Such improved patients can sometimes be made to walk without a crutch by means of a thick soled shoe; or, if that be insufficient, an artificial thigh case, with its fixed point on the tuberosity of the ischium, may be fitted on, with side splints ending in a wooden leg or artificial foot. *Prag.—Vtljschft.*, 1853, Band II.

A NEW MODE OF TREATING PURULENT OPHTHALMIA.

By M. DeConde.

M. De Conde, a Belgian military surgeon, dissatisfied with the present treatment of this disease, which seems to be very prevalent still in the Belgian army, lays down a new plan, based upon the following considerations:

1. The greatest danger to the eye arises from the contact of the upper lid, which, inflamed and swollen, floods its surface with

an acrid and corrosive pus. The excessive heat of the eyelid the internal surface of which is rough and unequal, interferes with the nutrition of the cornea, while the acridity of the pus leads to its softening and destruction. 2. It is admitted that it is of importance to prevent the contact of inflamed mucous membranes by the interposition of an isolating body, such as sharpie or wadding. This is seen in vaginitis, balanitis, and fissure of the anus. 3. This body, which may alone produce great amelioration, or even in some cases a removal of the disease, may exert a powerful effect if impregnated with an active agent. In this way lint, soaked in a concentrated solution of acetate of lead, and placed between the glans and the prepuce, will cure gonorrheal balano-posthitis within forty-eight hours. 4. Cod liver oil exercises a powerful action in disease of the mucous membrane, modifying and then suppressing their secretions. It strengthens the fibrous tissues of the eye and the cornea, and tends to prevent ramollissement. It is especially in ulceration and chronic ramollissement of this membrane that this double action is perceived. 5. The red precipitate ointment (four parts to fifteen of lard and fifteen of linseed oil) is an excellent substitutive agent, sufficing alone to arrest the disease when employed early. It is the best remedy for cutting short the ophthalmia of new-born infants. 6. A solution of the chloride of lime (thirty parts to two hundred of water) is an energetic modifier, neutralizing with certainty the virulence of the secretions. 7. Lastly, perchloride of iron exerts an instantaneous hæmostatic effect upon the hæmorrhagic mucous membrane, and an induitable modifying influence upon the mucous secretion.

As an isolating body, M. De Condé, after trying various substances, gives the preference to wadding, as forming at once an inoffensive application, capable of imbibing and favoring the flow of the secretions, and of being impregnated with medicinal agents. A slip of the wadding somewhat longer than the transverse diameter of the eyelids, is brought opposite the palpebral aperture, and gently pushed up beneath the upper eyelid by means of the little finger or a large probe.

ON THE ABSORPTION OF ABSCESSES.

M. Chassaignac brought this subject recently under the notice of the Paris Surgical Society, stating his belief that purulent collections are never absorbed, at least the solid globular portions, although the serum may sometimes be so removed. Abscesses said to be so removed were in fact non-purulent collections or lymph. M. Richard pointed out the error of supposing that pus-globules were not absorbable because they were solid, inasmuch as tumours, effusions of blood, &c., are known to undergo such absorption. It is by no means rare to find small axillary or peri-mammary abscesses becoming absorbed; abscesses the size of a pigeon's egg, will in chronic farcy disappear in twenty-four hours; and congestive abscesses not unfrequently heal without any aperture having been made. M. Huguier thinks every one must have seen examples of the spontaneous disappearance of abscesses, a by no means rare circumstance, in inguinal bubo. M. Verneuil referred to a case that came under his care, in which an undoubted abscess, as large as the fist, situated in the middle of the thigh, entirely disappeared after three weeks local application of iodine. In another case, two abscesses in the axilla were opened, while two others, just as manifestly fluctuating, being left to themselves, were spontaneously absorbed. In a case of hygroma, M. Morel-Lavellée obtained a few drops of fluid by means of an exploratory puncture. This was shown to be pus, and the rest of the fluid being left in the collection, flying blisters were applied, and entire absorption was induced. M. Chassaignac observed that this was the only demonstrative fact advanced in the discussion, and he must wait for others to be convinced. At present he does not believe that properly characterized abscesses are capable of absorption. Many cases are, it is true, mentioned by authors, but with insufficient proofs. Fluctuation and other symptoms mentioned are not sufficiently rigorous ones. Thus, in a case of angioleucitis, in which he opened two collections apparently alike, one contained pus and the other plastic lymph; and from some inguinal bubos he has opened, he has only removed such lymph. M. Broca, however, desired to know what M. Chassaignac designates as "collections of plastic lymph;" for if he means those masses of yellowish concrete semi-solid matter found in

the centre of commencing bubos, M. Broca can assure him that this substance, which is susceptible of absorption, contains an enormous quantity of pus globules. M. Collerier declares that since he has treated bubos by the method recommended by Dr. Sirus-Pirondi—viz., blistering, followed by tincture of iodine, he has obtained a cure by absorption of the pus in nine-tenths of his cases. Even when the blister does not succeed, it still exerts a great influence upon the absorption of the globules; and if the bubo has afterwards to be opened, pus containing a large proportion of serum is discharged.—*Gaz. des Hopitaux, and British and Foreign Med. Chir. Rev.*

PHYTOLACCA DECANDRA—POKE ROOT—IN GRANULAR CONJUNCTIVITIS.

The readers of the North American Med. and Chir. Review, Jan. 1857, need scarcely be reminded of the valuable paper of Dr. C. S. Fenner, of Memphis, Tennesse, on granular conjunctivitis, and of the high opinion which its author entertains of the phytolacca, in the form of tincture or of strong decoction, in this disease.

Dr. Parry, of Indianapolis, had at a previous period made use of the poke root in the case of a patient suffering under rheumatism conjoined with conjunctivitis. In a short time both diseases yielded to this treatment.—*Ibid.*

THE CAUTERIZATION OF STRICTURES OF THE NASAL CANAL, BY MEANS OF CATGUTS IMPREGNATED WITH A SOLUTION OF NITRATE OF SILVER.

By Dr. Raw, Professor of Medicine at Berne.

Ten years ago I conceived the idea of employing this means for cauterizing contracted or indurated canals; I first made the experiment on the Eustachian tube, and afterwards on the urethra, and on the nasal canal. *Process:* The cat-guts are cut into pieces of convenient length, polished at one extremity, which is smoothed with a very fine file, then washed in a solution of potash to free them from the oil which they contain. They are

afterwards plunged into a strong solution of nitrate of silver (1 part to 10 of distilled water,) from which they are taken at from four to eight hours, according to their thickness; they are then dried by suspension in the open air in a dark place. If the cat-gut is left too long in the solution it will become twisted and distorted. After drying, the surface and the interior are saturated with crystalized nitrate of silver. Should there be any inequalities they must be removed by the file. The pieces of cat-gut thus prepared, are preserved in blackened glass-tubes. In course of time the color of the cat-gut becomes darker, without the curative effect being weakened.

When we wish to use these cat-guts in strictures of the nasal canal, they are flattened at the upper extremity so that they can be applied and fixed to the fistulous orifice at its outlet. After performing some abstergent injections the cat-gut is introduced, without the coat of oil, until it becomes strongly pressed in the contraction. At first the patient experiences scarcely any pain, and that which he feels afterwards has never been so great as to compel me to withdraw the cat-gut before the lapse of twenty-four hours. If the irritation is not too great, introduce every day a new cord, after having previously injected with fresh water, or what is better, a solution of sulphate of copper. After the second trial we perceive that we may increase rapidly the diameter of the cords. When the irritation becomes too great, which rarely occurs, we introduce cat-guts, not nitrated, until the irritation ceases. When the canal is sufficiently dilated throughout its extent, and the secretion has lost its purulent character, it is well to wear for some time a leaden nail, before allowing the fistulous opening to close. The strictures of the nasal canal not caused by an anomaly of the osseous duct, heal by this method in a few weeks, while we know the other methods often require several months. I have also in several cases successfully used the solution of sulphate of copper.—*Charleston Med. Journal and Rev.*

NEW OPERATION FOR HARE-LIP.

Allen Duke makes this operation by paring the edges of the fissure obliquely from before backwards, and introducing the

sutures, armed by two needles, just behind the skin, through the rest of the thickness of the lip, and tying on the posterior surface of the lip. The removal of the upper suture is facilitated by bringing out the ends of the angle of the mouth and securing with adhesive plaster.

Where there is fissure of the jaw, the projecting portion is not completely severed, but, just enough to admit of its being used to fill up the space, the edges being pared and sutures introduced before the lip is operated upon.

He reports, in the *Lancet*, four operations upon children, whose ages varied from six weeks to four months.

MIDWIFERY.

BENEFICIAL EFFECTS OF PEPSINE IN THE OBSTINATE VOMITING OF PREGNANCY.

By L. Gros, M. D.

In the great majority of cases the vomiting of pregnancy may safely be left to the influence of time; but there are some cases in which females are scarcely able to retain in their digestive system a sufficient amount of nourishment to support their existence, and are therefore reduced to the last degree of emaciation. In some, also, the shocks occasioned by this obstinate and repeated vomiting become the source of abortions, which might have been prevented by moderating the activity of the morbid phenomenon. A very remarkable case was related in 1856, by M. Teissier, Professor of Clinical Medicine at Lyons, showing the immediately beneficial effect of a dose of pepsine in a case of vomiting during pregnancy. In this case the symptoms resisted all the ordinary methods which were employed, and the patient was unable to retain in her stomach any substance whatever. Under these circumstances, the patient was brought to M. Teissier, who found her in the following condition: The vomiting had continued for two months, and she was at the end of the fourth month of her pregnancy; she presented the appearance of a skeleton, having the aspect and the cough of a phthisical sub-

ject; the pulse was 140, and M. Teissier thought at first that the case was one of pulmonary tubercle. Finding that all treatment had been hitherto inefficacious, and that the lady was actually dying of inanition, he was seriously meditating upon the propriety of inducing abortion as a means of saving her life: but as a last resource before operating, he determined to employ pepsine. He accordingly prescribed one gramme, to be divided into two doses, and take every day in a spoonful of broth. At the very first dose the broth was retained, and from that moment the vomiting never returned. On the third day the lady ate some chicken, and then some beef-steak. The treatment was continued in the same manner for three weeks, and at the end of that time the cure was complete: the emaciation was replaced by *embonpoint*, the fever and the cough ceased with the vomiting, and at the end of the ninth month, the lady was safely delivered.

Dr. Gros then relates six other cases in which the pepsine was employed with the same success, and he thinks himself warranted in concluding that pepsine undoubtedly produces good effects in the vomiting which attends pregnancy. He explains the results by supposing that, although in the first instance the vomiting is due only to the sympathy existing between the uterus and the stomach, yet subsequently the stomach itself becomes affected, as is proved by the fact that in the beginning of pregnancy the vomiting occurs only in the morning or the evening; but in aggravated cases it supervenes after every meal, and all alimentary matters are rejected. In such cases, therefore, when the stomach has taken on a morbid habit, and exhibits an alteration of secretion, the pepsine appears to be really indicated; although in a merely sympathetic action between the uterus and stomach it would be difficult to explain the efficacy of its action.—*Brit. and For. Med. Chirurg. Rev.*, from *Bull. Gén. de Thérapeut.*

IODIDE OF POTASSIUM FOR DISPERSION OF THE MILK.

M. Roussel, the professor of clinical midwifery at Bordeaux, having observed the effect of iodide of potassium in diminishing the milk when given in the non-puerperal condition, resolved to administer it in cases in which the dispersion of this secretion

was desirable. A woman, who suffered from bad chapped nipples, had great and very painful engorgement of the breasts, attended with much fever. The iodide was given, and by the next day the pain and fever had disappeared, its employment for three days rendering the cure of a tumefaction that threatened abscess complete. M. Roussel has since then tried it in twenty cases, and always with success. After the cure, the milk returns again two or three days after the suspension of the iodide. Its action is more decided in the dose of from six to eight grains per diem than when given in larger quantities. The excessive secretion of milk may be prevented or moderated by administering it on the first or second day after delivery.—*Gazette des Hôpit.*

VESICO-VAGINAL FISTULA IN EUROPE.

Dr. Bozeman, in a letter to the junior editor, dated Dublin, August 26th, thus writes of the *button-suture :*

"While in Edinburgh, Dr. Fleming, Surgeon to the Richmond Hospital, found a case with fistula, and announced to the class that I would operate to-day; but, much to our disappointment, the patient became frightened, and concluded not to submit to the operation. Not a case of this description has ever, as far as I can learn, been cured in Edinburgh. I operated twice while there; once in the Royal Infirmary, and once in the practice of Professor Simpson. The case in the Infirmary turned out badly, dying on the sixth day from cellular inflammation. I was in the north of Scotland when this occurred, and you can well imagine my surprise upon returning. Fortunately the parts were preserved for me, and the result of the examination proved satisfactory to all present, viz.: that the operation, as far as it was concerned, was entirely successful. The fistula was completely closed, and the suture apparatus was perfect in its adjustment. Not a drop of urine escaped from the time of the operation. The case was a very bad one, but I never performed an operation with which I was better pleased. The cervix uteri was shut up in the bladder, and the operation consisted in restoring it to its natural position in the vagina, and in that way closing the opening. Prof. Simpson and all present were delighted with the procedure.

The patient, I have since learned, was one of the greatest drunkards in Edinburgh, and was several days before the operation, beastly drunk. Under these circumstances no other termination could well have been expected. I performed an operation in the Royal Infirmary at Glasgow, but have not yet heard from it. I received a letter from Mr. Erichsen, while in Edinburgh, stating that the case which I operated upon in London was cured."—*N. A. Medico-Chirurgical Review.*

DYSMENORRHÆA CURED BY STRAMONIUM.

By A. P. Merrill, M. D.,

Of Memphis, Tenn.

A married woman, aged about thirty years, applied to me for assistance some two years ago. She stated that she had suffered severely at every menstrual period from the age of sixteen years; that she had been prescribed for by several eminent physicians, and had tried almost every form of quackery, and secret nostrum, without obtaining relief. Since her marriage, about a year previously, she had suffered more than ever, and from a specimen of her suffering which I then witnessed, I judged that her case was one of unusual severity. I prescribed the extract of stramonium, to be taken for several successive days, before the expected attack. She took the remedy only for a day or two, and suffering much from narcotism declined its further use, although she believed it had afforded her some degree of mental relief. About a year afterwards she consulted me again, stating that her sufferings at her regular period had greatly increased, so that in some instances her life had been supposed in great danger. As she had already tried a great variety of remedies without benefit, I determined to make a more thorough experiment with the stramonium, which I had used successfully in less obstinate cases. She was, therefore, directed to begin with the extract ten days before the expected paroxysm, and take one grain every three hours, diminishing the dose in case of too great narcotism, but without lessening the frequency of its repetition. The next period was passed with suffering comparatively slight, and sev-

eral succeeding periods with none whatever, with an increase in the menstrual secretion, and an anticipation of time. After five or six months of such treatment, the doses being reduced to half a grain, she became pregnant, and in excellent health.—*N. O. Medical News and Hospital Gazette.*

THERAPEUTICAL RECORD.

Croup.—Much discussion has lately taken place in Paris respecting a bold measure in croup, viz.: actual cathertism of the larynx and trachea, followed by caustic injections, proposed and practised by M. Loiseau, of Montmartre. This operation obviates the necessity for tracheotomy, and is warmly supported by M. Trousseau in a report to the Academy of Medicine.

Pain— A trial was lately made at the University College Hospital, London, of the plan recommended by Dr. Alex. Wood, of Edinburgh, in 1855—namely, of injecting a few drops of morphine over the seat of the affected nerve in cases of excessive local pain. In the case alluded to, the supposed remains of a rusty nail in the back of the hand were attempted to be removed but the severe pain which had before existed continued after the operation. By means of a small syringe, about twelve drops of the solution of morphine were injected beneath the skin (the old wound having perfectly healed,) close to the seat of pain. No amelioration followed; on the contrary, both immediately after the injection, and on the next day, the pain was increased.

Poisoning (rhus radicans).—An aqueous solution of the nitrate of silver, sprinkled over the part, has been used with success in this affection. Dr. Dakin, (Am. Journal of Medical Sciences) gives the following most reliable prescription.

℞ Sulph. Cupri. }
Precip. Mer. Rub. } - - - - - aa ℥j.
Terebinth. Ven. - - - - ℥jij.
Axunge porc. - - - - - ℥j.
Fit. Ung.

Gonorrhœa.—Dr. Heustis recommends the following method of treating gonorrhœa, as one which has proved successful in his own practice. ℞. Creasot., gtt. x.; acid tannic., gr. x.; aquæ, oz. iv. M. Inject. four times a day, retaining each injection a minute or two—washing out the urethra with cold water before using it. In connection with this, the following balsam-mixture is used. "℞. Copaiba, spts. æther nitros, spts. lavend. comp., aq. calcis, each one ounce; tinct. opii, two drachms; sacch. alb., six drachms; ft. mist. S. Teaspoonful morning and noon, and two teaspoonfuls on going to bed, first shaking the mixture well each time."

Jaundice—Dr. Stule (Chicago Medical Journal) gives the following as a prescription for jaundice:

℞.	Pulv. Doveri, - - - -	30 grs.
	Pulv. nit. potassa. - - - -	30 "
	Sub-murias hydrarg. - - -	10 "

Mix; divide into six powders, and give one every four hours until all are taken.

Pneumonia (Typhoid).—In a report on practical medicine, to the Illinois State Medical Society, we find the following prescription for the first stage of typhoid pneumonia.

℞.	Sul. Quinia, - - -	grs. xvj. to xx.
	Calomel, - - -	" xv.
	Pulv. opii, - - -	" ij.
	Ipecac. - - - -	" j.

" Divide into four powders, and give one every three hours.

Prolapsus Uteri—Dr. Bonordon observes that as prolapsus uteri usually arises from hypertrophy of the organ and a relaxed state of the round and broad ligaments, the indications are to remove the hypertrophied condition, and to strengthen the ligaments. In two cases he has been enabled to completely fulfil them by internal remedies. He administered twenty drops of *tr. ferri. mur.* morning and evening, giving with the evening dose also three gr. of *secale cornut* and ten gr. of *gum galbanum*, the external parts of generation being well rubbed several times a day with Hofmann's *balsamum vitæ.* At night, the patients were

directed to lay with the pelvis somewhat raised. The secale was continued for fourteen nights, next alternate nights, and then a while at longer periods.

Asthma.—Dr. W. T. Ousley (*Nashville Med. Journal*) states, that he has used iodide of potash in asthma with advantage. He gives iij. grains, dissolved in sweetened water, every two hours until the patient is relieved.

Strabismus.—Mr. Walton operated recently at the Central London Opthalmic Hospital, on a girl six years old, for single internal squint of the right eye. Mr. Walton proceeded to make the small incision in the conjunctiva, close to the cornea, according to his accustomed method, when a peculiar adhesion of this membrane to the eyeball attracted attention. It required unusual care to open it sufficiently, and the preternatural intimacy between the parts was an obstacle to the ready passage of the hook for the securing of the muscle. Mr. Walton showed that there was no external evidence of the existing peculiarity, which should, so soon as detected, make an operator exercise more than ordinary care in his proceedings; and also that in this, and all like instances, it was impracticable to divide the muscle sub-conjunctivally. He alluded to the uncertainty of this method as devised by M Guèrin, even in ordinary cases. The operation of tenotomy, as practised for distortion in the limbs, was not in like manner applicable to strabismus.

Uterine Hæmorrhage.—Dr. Wittcke strongly recommends the application to the whole abdomen of cloths or handkerchiefs well moistened with a lotion composed of weak spt. menth. pip., spt. cinnam. ää ℥vj. aceti ℥viii. At first, they excite a marked impression of cold, to keep up which they have to be reapplied every five minutes. During the second hour, as a general rule, they should be changed only every ten or fifteen minutes. The womb contracts powerfully, the bleeding and faintness cease, and after the application has been continued for six or eight hours, a general warm sweat breaks out, which is a sign that all danger is over. With these applications are occasionally combined two or three doses of a powder consisting of secale and cinnamon ää five grs. If the spirit is not at hand, to a pint of infusion of

peppermint may be added half a pint of vinegar, and the same of spirits of wine. On an emergency, too, we may mix one part of water with half of spirit and half of vinegar; but the restoration of strength and the sweating are not so readily produced. .

Sick Headache.—Dr. J. C. Norton, of Homer, Minnesota. in a report on the botany of Winona county, says of the cicuta: "In small doses, it is, according to my experience, a sure cure for nervous sick headache."

Inversion of the Uterus.—Dr. White reports to the Buffalo Medical Association, a case of inversion of the uterus of over fifteen years standing, which he has succeeded in returning. Ordinary manipulation was employed, and the operation lasted only fifteen minutes.

Yellow Fever.—Dr. Fenuer reports, that he has used both chlorine and veratrum viride in yellow fever, and that he has found them of decided benefit. The terrible mortality of the late epidemic, furnishes a most telling commentary on this statement however.

Diptheritis.—Dr. Campbell (*Southern Med. and Sur. Journal,*) recommends for this disease the internal exhibition of from 5 to 15 grains of quinine, in divided doses, during the earlier part of each day for several days, and the local application of powdered alum to the throat.

Tooth Ache.—The following is from the "Revue de Thérap. Med. Chir."

℞.	Chloride of zinc, - - -	grs. 10
	Perchloride of iron, - - -	" 10
	Alcohol, - - - - -	" 40
	Distilled water, - - - -	" 40
	Muriate of morphia, - - -	" 1
	Essence of gilly flowers, - -	" 10

A few drops on cotton wool, when inserted into the tooth, affords immediate relief.

Pessary.—Mr. Walter Bryant recommends a new, simple, inexpensive, and very efficient form of pessary, devised by him

for cases requiring uterine support, or the employment of medicated substances in the vagina. The pessary is composed of cotton wool, encased in a pyriform net. It is softer, less irritating, and far more cleanly than the sponge or caoutchouc pessary, and readily absorbs astringent or other solutions which may be prescribed. The mouth of the net is closed by a stout silk thread, which hangs through the vagina, and allows of the ready withdrawal of the pessary.

Speculum.—In some clinical observations made by M. Velpeau, at the Charité Hospital, upon a case of supposed malignant ulceration of the neck of the uterus, in the course of his remarks the lecturer stated that professional men generally overrated the diagnostic powers of the speculum; and that in nine cases out of twelve we can very well dispense with it. Both experience and reason tend to render the use of the speculum less frequent. This instrument, in fact, teaches us nothing respecting the volume, shape, position, or density of the organ to be examined. The only circumstance which it brings to light is the tint of the cervix, and the knowledge of this tint is often of no use whatever. The finger, M. Velpeau thinks, is far more useful; with it we make out ulcerations, granulations, fungosities, and the consistence of the cervix. Nay, the speculum often leads into errors of diagnosis, and makes us suspect lesions which have no existence. The neck of the uterus, caught by the extremity of the speculum, appears larger than it really is, and the os looks gaping; nor should it remain unmentioned, that a simple fold of the vagina is often mistaken for the cervix, and the caustic is applied where it was not intended to act. M. Velpeau, without rejecting the speculum, wishes, however, that its use may be considerably restricted, and that young practitioners should get accustomed to establish their diagnosis by means of digital examination.—*La France Medicale.*

Hemorrhage—Dr. Wm. Hauser (*Atlantic Med. and Sur. Jour.*) says that a solution of sulphate of zinc is an excellent styptic. He puts a teaspoonful of the salt into a cup of cold water and applies the solution to the bleeding point.

Pepsin Wine.—Take of starchy pepsin, prepared according to Messrs. Covisart and Bourdault's formula, one drachm and a half; distilled water, six drachms; white wine (of Lunch.) fifteen drachms; white sugar one ounce; spirit of wine (33°), three drachms. Mix until the sugar is quite dissolved, and filter. One tablespoonful of this wine contains about fifteen grains of pepsin, and may be given after every meal.

Hypertrophy.—The Hypertrophy of any tissue can be most successfully treated by the following prescription:

℞. Potass. iodidi. - - - - grs. viii.
Hydrarg. chlorid. corrosiv. - - " 1–12

This dose to be repeated three times daily; and to be followed on every third night, by a purgative composed of compound extract of colocynth, jalap and mercury.

Pneumonia—Dr. A. Jacobi—*New York Journal of Medicine*—recommends the Oxsulphuret of Mercury in Pneumonia, especially of children. He gives a grain or two every two hours, increasing the dose in proportion to the age of the patient up to four grains.

Puerperal Convulsions—Dr. E. N. Chapman states that in Puerperal Convulsions he first bleeds freely and then administers ¼ grain of tart. emetic every ten minutes until the patient vomits freely three or four times; he then keeps up the impression by giving from ¼ to ½ gr. every hour.

Anasthesia—M. Jacowski, a practitioner of Paris, has revived the practice of compression as a means of preventing pain. The compressor employed consists of two parts, connected by a steel spring very much like a hernial truss. The spring is passed behind the head, and the pads are applied either within the meatus auditorius on each side, or behind the rami of the jaw in front of the ears.

Dysentery—Dr. L. B. Brown gives the following for Dysentery:

℞. Creosote, - - - - - gtt. x.
Ascetic Acid, - - - - gtt. xx.
Morphine, - - - - - gr. ij.
Distilled Water, - - - ʒj.

A teaspoonful every three or four hours.

Glucosuria—The Union Médicale, reports of five cases of diabetes cured, or much benefitted, by the use of mineral waters of Pouges, in France (which contained the carbonates of soda, lime and magnesia,) under the care of Dr. De Crozant. He considers the affection to result most frequently from disease of the liver, which opposes the passage of the glucose derived from the starch in food, causing it to be taken up by the supplementary abdominal circulation, which conveys it to the kidneys.

Enlargement of the Spleen—A writer in the *New Orleans Medical Journal* recommends the following for this affection:

R. Potassi iodidi, - - - - ʒvj.
Spiritus Lavendulæ, - - - ʒiv.
Aquæ Cinnamonia, - - - ℥vj.
M. S. A teaspoonful three times per day.

This preparation of Iron must be given at the same time:

R. Ferri et quiniæ citras, - - ʒij.
Confectiones Rosarum, - - - q. s.
M. Sixty pills. S. Two, three times a day.

Lime in the Eye—The *Indecateur de Mayence* states that a strong solution of sugar in water, dropped into the eye, is an excellent remedy for the severe injuries to that organ caused by the accidental contact with particles of lime. The solution has the property of dissolving and removing the lime.

Yellow Fever—Drs. White and Ford, of Charleston, S. C., have made use of the veratrum viride and gelseminum sempervirens in the treatment of yellow fever during the late prevalence of the epidemic in that city. Their confidence in them as arterial sedatives is such that they will continue their use.

Electric Anœsthesia—Mr. G. Waite, in the London *Lancet*, says:

"A few years prior to the Great Exhibition of 1851, Mr. Laxton registered for me the right to patent the electro-galvanic current for surgical purposes, its usefulness having struck me when asked by patients to allow them to hold chains, consisting of the metallic combination of various galvanic elements, when

undergoing dental operations. Subsequently, at the Great Exhibition, I exhibited a battery, with the chain and wires, ready for the dentist's use."

Warts—In the American Journal of Pharmacy, Dr. C. O. Monelle, of Baltimore, gives the following method of extirpating these unsightly excrescences: "I first cauterized the wart with lunar caustic, sufficiently free to reduce it to a level with the surrounding cuticle, and then upon the exposed surface, dropped from three to five drops of the tinct. iodine; this I repeated three or four times daily for three days, and then applied a dressing of lint and simple cerate, which healed the sore very rapidly."

Rheumatism—Dr. Awenarin, (*Medical Zeitung*) states that Propylaminum, which is obtained from herring-pickle, cod-liver oil, human urine, etc., is an admirable remedy in rheumatism. His formula is:

R. Propylaminum, - - - gtt. xxiv.
Aq. Font., - - - - ℥ij.
M. A tablespoonful every two hours.

Erysipelas—Dr. John G. Stephenson, of Terre Haute, Indiana, desires to call the attention of the profession (Western Lancet, May, 1857,) to the treatment of erysipelas which has proved so beneficial in his practice that the use of it has become, with him, a matter of routine. "The treatment is simply the covering of the inflamed surface with wet tobacco leaves (such as are to be had in any cigar shop.) which are permitted to remain until much nausea is produced."

Piles—This ointment is advised in Piles:

R. Ac. Pl., - - - - - ℥j.
Lard, - - - - - - ℥j.
Nitric Acid, - - - - gtts. viii.
M. Apply morn, noon and night.

Boils—The following mixture as an aperient tonic is the one usually prescribed for patients suffering with boils: R. Sulphate of magnesia ℥iij, sulphate of iron ℥ij, dilute sulphuric acid ℥ss, diluted with infusion of quassia to a pint in quantity. Of this the dose is from two to four drachms three times daily

taken in water. Locally each boil is touched with a glass brush dipped in the nitrate of mercury solution, and is subsequently dressed with an ointment containing a small proportion of the ammonio-chloride, or some similar salt of mercury.

Antidotes for Arsenic—M. Nonat (*Arch. Gen.*) has first reported the results of his experiments upon the Salts of Iron, considered as antidotes of Arsenic Acid. They are the following: [1] After simple ligature of the œsophagus, two dogs, upon whom it was practised, survived from six to seven days. [2] Arsenic Acid when given alone, produced death in from two to twelve hours. [3] Dogs which were given Arsenic Acid, contemporaneously with the protoxide and the deutoxide of iron, died as speedily as if no antidote had been exhibited. [4] Of thirteen dogs dosed with Arsenic Acid and the hydrated peroxide of iron, the mortality was in exact proportion to quantity of the antidote given; when small, death occurred in a short time, and *vice versa.*

Aneurism.—M. Vauzetti of Padua, in a report to the French Academy of Sciences, states that he has cured seven cases of Aneurism by compression with the hand alone, without causing pain to the patient, or producing any serious accident. He believes that this method of treatment will eventually supercede that by mechanical appliances.

Forces of the Circulation.—M. Vaurer, after a series of investigations, reports, that there are three distinct forces which influence the circulation. (1) A primitive force, the nature of which is unknown, seen in the egg before the heart is formed. (2) The contractions of the heart propelling the blood through the arteries. (3) A general compression, which acts upon the blood contained in the capillaries and the veins.

Cancer of the Breast.—M. Velpeau, in presenting the second edition of his work on "Diseases of the breast," to the Academy, observed, that he had conclusively established the following propositions, to wit: (1) True cancer when left to itself, or attacked alone by the resources of pharmacy and hygenie, is never cured, but produces death invariably. Those who pretend to the contrary either deceive the public intentionally, or are self deceived. (2) A number of true cancers are cured permanently by the use of the knife or caustic.

Spontaneous Gangrene—M. Trudeau, recently discussed this subject before the Academy of Medicine at Paris, and established the following propositions. (1) Arteritis is one of the most frequent causes of spontaneous gangrene. (2) It can be advantageously treated at first, by emollients, antiphlogistics and opium. (3) In cases of sphacelated members, an effort at spontaneous elimination increases and prolongs the danger. (4) This delay renders the consecutive operation more uncertain. (5) An amputation made in time, abridges suffering and diminishes the danger. (6) Though an extreme measure it is justifiable, unless there be decided contra-indications. (7) The embolic theory is applicable in some affections of the heart and arteries.

EDITORIALS AND MISCELLANEOUS MATTER.

MEDICAL ETHICS.

Much discussion has recenty taken place upon the subject of Medical Ethics. Volumes have been written in exposition of it; codes have been promulgated, pregnant with all the learning of the Nestors of the profession, and appealing to the most honorable principles of human nature for sanction and approval. Clauses have been continually inserted, providing against every contingency in which the interests of practitioners can be brought in conflict, and so arranged, as to serve, under all circumstances, as sign posts, to direct those interested, out of the wrong way and into the right one. But, unfortunately for medicine, no practical and permanent advantage has resulted from this protracted excitement, and instead of fraternity and friendship, ill feeling and hostility exist, in too many instances among the followers of Æsculapius. The elements are yet in a condition of confusion and disturbance. The angry waters are boiling and surging as of yore. The disagreement of Doctors, is still a source of amusement to others, and of disgrace to themselves. No Solon or Justinian has arisen to establish laws, so just,

equitable, and imposing, as to recommend them immediately to the cordial admiration and entire approval of the medical world. No "brighter day" has dissolved the mists of selfishness and prejudice which have so long hung heavily upon the profession. Physicians can be readily found who are ready to pander to the prejudices of the multitude, to use the most disreputable articles for their own advancement, and to detract from a professional brother without shame or compunction, for the sake of a little ephemeral distinction, or a few dollars and cents. Some of the fraternity will scruple at nothing however mean, low, or contemptible; will practise the most shameless and disreputable arts; will cringe and fawn and villify, just so far as their pusillanimity permits them to go, or opportunities present themselves for the cowardly stabs of such miscreants. Professional success, that is securing patients, is considered the *summum bonum* of existence; and for it, honor is sacrificed, the most sacred obligagations despised, and tricks attempted which would disgust the "lowest juggler who ever threw his balls." The rich are flattered and caressed; the poor courted and petted; the virtuous treated with reverence and respect; the vicious approved and smiled at; and every popular breeze wooed with such adroitness, that not a breath escapes the distended sail, which flies before it—perhaps to its own destruction. But this is not all, a regular war is made upon rival practitioners, in which no civilized law is observed, and whose watchword is, "every advantage and at any cost." We might notice in detail, the means employed by these persons to effect their purposes, the appliances by which the eye of the community is dazzled, the arts used to publish marvellous successes to the world, the schemes invented to injure professional brethren, and all the machinery by which pretension and meanness seek to advance themselves in public estimation, not to mention the flatteries exhausted upon twadling crones, or the hypo-

critical cant invested in religious demonstration, but the task is too disgusting to be attempted by a decent man for respectable readers.

This condition of things, results from two passions, which, unfortunately, find but too genial an atmosphere in the human heart; we mean avarice and vanity. The scriptures have long since declared, that, "money is the root of all evil," and the experience of mankind has amply confirmed the truth of the maxim. For it men are willing to sacrifice the ties of blood and kindred; to stifle every generous sentiment and ennobling principle within their bosom; to despise honor, truth and justice; to trample upon the laws of God and man; to forget the endearments of home, and to drive the plowshare over the graves of their fathers; to spend long years of dreary toil amid the snows of Siberia, the fervid heat of India, or the lonely wilds of California; to brave the dangers of the ocean's traitorous waves, and the mysteries of its hidden caverns, and to count no sacrifice too great, no labor too onerous and no crime too appalling in the accumulation of its golden treasures. Avarice feeds upon every acquisition, grows with each indulgence, and strengthens upon the very longings it inspires. It enters the heart a helpless infant, but is speedily transformed into a giant, whose heavy heel crushes out the virtuous sentiments implanted there, and compresses all beneath it to the consistency of iron. It first closes gently the too generous hand, and then binds it with such unyielding cords, that not even the strongest appeals of poverty and wretchedness can break them. In a word it takes possession of a man's soul, reduces his highest powers to a state of abject servitude, and tyrannizes over his entire nature.

When this passion actuates a physician, it unspheres him, places him in an orbit whose centre is *self*, breaks the link which bound him to a more elevated destiny, and paralyzes his useful-

ness as a member of the profession. He graduates his sympathy and attention by the prospect of gain. He only enters the sick man's room to thrust his hand in his purse. He hears but the chink of the dollar in his "auscultations." His "explorations" are all after gold. His "percussions" have the ring of the *real metal* about them. And his prescriptions are redolent of *mint-drops*, suggestive of *heavy draughts*, and are put up more for the benefit of the doctor's pocket than the patient's stomach. In fact, his mind is nothing but a ledger well posted; his heart an iron chest with money in it; his degree a false advertisement, and his profession his stock in trade; whilst the contempt and enmity of every respectable brother, is the sure reward of the miserable passion which has so depraved him. He may succeed in accumulating money; but death finally closes the scene! And he goes to his last account without carrying with him the consolation of a good conscience, or leaving behind an honorable name for his children.

We can but quote the words of the poet:

"May his soul be plunged
In ever-burning floods of liquid gold,
And be his avarice the fiend which damns him."

All have vanity to some extent, but we are convinced that physicians possess more of it than any other class of men. All have their admiring friends, who esteem them more learned than was Hippocrates himself. Each one is an autocrat within his particular sphere. So many cures are effected through the benignant ministrations of nature, and in despite of doctors and drugs, that the veriest tyro imagines himself the embodiment of all the skill and attainment known to the profession. The lawyer's effort is made in public, before bench, bar and jury, so that an accurate estimate of his talents and qualifications immediately obtains, before which he sinks or rises until his proper level has

been reached. The minister of the Gospel is constrained to submit to the same searching ordeal; and soon learns to graduate his self-esteem by the general estimate of his abilities. But the physician struggles with disease in private, with no competent spectator of the scene, under circumstances which render his will absolute, and his slightest word the voice of destiny; surrounded by those whose anxieties make them credulous and confiding, and assisted by an overruling Providence, which comes to his rescue in an extremity, either by restoring the patient to health or consigning the victim to that narrow resting place which covers up all errors and utters no accusation. His vanity feeds upon the pabulum which this accumulated responsibility supplies. His self-love expands under the stimulus thus afforded. His ideas of his own dignity and importance increase with each trial to which he is subjected. And he finally becomes that inflated, supercilious and pedantic specimen of professional development, by whom the rights of others are invariably despised, and medicine brought so frequently to an open shame. Such a man grieves over the loss of patronage, as a calamity and a wrong. He weeps bitter tears at the grave of friendship, not from real sorrow at the bereavement, but because of an apprehension, that in failing to effect a cure, his reputation may suffer in the estimation of the community. He can never be induced to mention a fatal case, save for the purpose of an egotistical explanation, or a sly insinuation that his medicines were improperly administered, and that he was called too late. He hates a funeral procession, bribes the sexton to toll the bell as quickly as possible, and pays "the devil" to omit all obituary notices of the patients who have died under his charge. He avoids consultations with his professional brethren, treats all physicians habitually as inferiors, and when forced into contact with them at the bed side, takes particular pains to whisper into the ear of some one interested in the result of the case, that but for his

timely intervention the sufferer would have been hurried to another world with the speed of lightning and the certainty of fate.

This picture may seem overdrawn, but we are confident that its original can be found within a stone's throw of any man who will seek for it; and we only hope none of our readers may discover in it their own counterpart.

We are so far from having that confidence in the perfectibility of human nature, which has distinguished the eutopian philosophy of these latter days, that we utterly despair of the complete moral regeneration of the race. The evils which we have mentioned originate in that natural source of depravity upon which the seal of corruption has been impressed by the hand of Divinity itself. They are the fruits of that forbidden tree which tempted and depraved our first parents in the garden. They are the legitimate offspring of those base passions wherewith humanity was cursed at the fall. Their complete eradication can only be effected by revolutionizing man's entire being, by elevating and purifying his nature, by destroying that selfish principle which possesses his heart, and by inaugurating that glorious millennium of virtue and happiness, whose coming has been proclaimed by the voice of inspiration. But, though perfection be impossible without the special interposition of Providence, improvement is always within the compass of human effort. If the profession cannot be entirely purged of its impurities, it may be made better than it is. Much can be done towards the elevation of its members, if it be attempted judiciously and persistently. Many of the abuses by which it has been disgraced may be corrected, if those interested in the maintenance of its dignity will make a united and disinterested effort in its behalf. And we now propose to sketch, hastily, the plan by which these most desirable ends can be effected promptly and effectually. A few preliminary observations, however, will be necessary to a proper

elucidation of our ideas, and an introduction of the subject. This is a fast age. Haste is the grand characteristic of the times. The value of every thing is measured by the celerity of its attainment. Society is only a *rush* and a *scramble*. The minds of men have become so habituated to the annihilation of space by steam and electricity, that they die of ennui unless time can be destroyed with the same rapidity. Youth does not wait for "its beard to grow," but is a man in taste and vices, when the rod of the pedagogue should be "teaching the idea how to shoot." Manhood courts the cares of age, and assumes its dignity, when the *toga virilis* is still an unfamiliar novelty. And old age, forgetful that it is appointed for man to die, occupies itself in devising expedients for the consumption of the few remaining hours of life. Medicine has felt this baneful influence, and yielded to it sadly. A physician's qualifications are determined by the rapidity with which his diploma was obtained. Schools have sprung up, whose only object seems to enrich themselves by striving to turn out the greatest number of doctors, and in the shortest possible time. They scramble for students as boys for marbles or ginger cakes. They pounce upon a fee with the rapacity of a hawk after his prey. They hurry through their courses and examinations with the speed of an "express train." They stuff their matriculates with such a mass of heterogeneous materials that no intellect is cormorant enough to digest them. And they send forth crowds of graduates, invested with all the dignity of the Doctorate and privileged to prescribe for the sick, but who have not one moral or intellectual qualification for the work before them. Their only ideas of the responsibility of their calling, or of the ethics of the profession, are derived from the well thumbed valedictory of some superannuated Professor; whilst the medical knowledge, with which they are to combat disease and make a name for themselves, is nothing more than that superficial smattering crammed into them by a quizer, or picked up at that *last* of *humbugs*, a public clinic.

These men go forth to the world as exponents of the dignity of medicine, without understanding the meaning of the term; to sustain its honor, without feeling more than a selfish interest in it; to be brought in contact with other physicians, without knowing their rights, or estimating the importance of such knowledge; to be tried by a code of ethics, without the least familiarity with them or the ability to appreciate their signification; and to involve the profession in the disgrace attendant upon their shameless acts, without compunction or regret. It is hardly necessary to assert our sympathy in those measures, which have been attempted so unsuccessfully, for the prolongation of scholastic terms, the extension of the curriculum, and elevation of the standard of graduation in all institutions where medicine is taught. These things would secure more knowledge of the science, and a greater success in the practice of physic; but they would conduce in no material degree to an observance of the ethics of the profession, or the inculcation of more elevated sentiments among its members. We propose, for the accomplishment of this end, the establishment of separate chairs in every school of the country, devoted exclusively to the great subjects of medical ethics, literature, history and jurisprudence. We would have professors appointed for the purpose of teaching all that relates to these important branches of a physician's education, so that he might meet the multitudinous responsibilities of his calling, properly prepared for them. We would have candidates for the degree examined searchingly as to their motives for desiring it, their ideas of its dignity, their acquaintance with its literature, their knowledge of its history, their ability to solve the complex problems of medico-legal science, and their acquaintance with those practical amenities and duties which should engage the attention and study of every cultivated practitioner. The good thus accomplished would be incalculable; and we trust to see the day when this plan will be perfected in North Carolina.

In the next place we would have boards of examinors appointed, delegated with ample powers to enquire into the moral as well as intellectual qualifications of those who come before them, and privileged to ostracize all against whom charges of professional impropriety could be substantiated by their peers. We would have them scrutinize the lives, habits and bearing of every aspirant, so that the question of his respectability might be satisfactorily settled. We would have them determine the motives actuating him, the principles acknowledged as his guides, and the views entertained by him of professional honor and dignity. In a word, we would have them ascertain whether he be a *gentleman* or not, and to reject him promptly if the slightest doubt existed on the subject. Those to whom certificates might be granted, and indeed all in the ranks of the profession, should be amenable to this board, to be tried faithfully by those composing it, and sentenced, if a proper case be made out against the accused, to an ignominious expulsion from the society and fellowship of more honorable practitioners. This idea of continued responsibility on one part, and of constant surveillance on the other, will doubtless prove distasteful to some—for

> No wretch e'er felt the halter draw
> With good opinion of the law.

But we are well assured that it would act as a most salutary restraint upon many, whose daily practices are a disgrace to their vocation, and who can be reached in no other way. The third plan, we would suggest for the improvement of medicine, relates entirely to the *morale* of those who are disposed to inveigh against the evils complained of, and who desire their eradication. They should despise every thing that bears the slightest semblance to unfairness or meanness. They should discountenance those who are continually disgracing the vocation, and dishonoring their titles; they should frown down quackery in every guise, expose pretension where ever it raises its specious form,

and hold the derelict up to public scorn, without exhibiting a fear or demanding a favor. They should remember that the profession is a common brotherhood, bound together by links of sympathy and interest, and so delicately organized as to respond in every part to whatever affects a single member. They should protect the rights of a rival as zealously as those of a friend, and defend his character and reputation with as much cheerfulness as their own. They should reverence their vocation, and diligently endeavor to qualify themselves for its high and holy offices. And, though jealous of their individual honor, and mindful of their legitimate interests, they should look beyond the contracted horizon of mere selfishness, scorn the allurements of every grovelling passion, and live for the honor and glory of that calling, which, when properly pursued, is perpetual incense to Heaven.

But as no creed yet promulgated by ecclesiastical prerogative has ever received the sanction of a people, unless it appealed directly to their religious sentiments, as no system of laws, however seemingly wise, salutary and equitable, or by what authority sustained, has succeeded in commanding obedience, unless addressed immediately to the ideas of right and justice pre-existing in the minds of those for whom they were designed, so no code of medical ethics can secure the permanent support of the profession, unless its members possess the moral attributes requisite for their appreciation.

To sum up, the alpha and omega of the whole matter is this, the evils which disgrace our calling in these latter times can be most surely abated by a rigid cultivation and practice of those cardinal virtues which constitute the character of a gentleman. And to all who desire the elevation of medicine, who mourn over its fallen greatness, who deprecate the ungenerous spirit which characterizes but too many of its followers, and who are really anxious to aid in regenerating, purifying and ennobling our be-

loved profession, we would say in conclusion, begin the work at home; cleanse your own bosoms, regulate the evil spirit of your rebellious hearts, prostrate that altar whereon the idol of selfishness has been so ardently worshipped, and let virtue be your guide. honor your friend, and truth your polar star.

> "Above all, to thine own self be true,
> And it will follow as the night the day,
> Thou canst not then be false to any man."

A CONTRAST.

We failed to send the first number of this Journal to the American Journal of Medical Sciences, because, from the difference in the size and price of that most admirable periodical, we did not suppose it possible to effect an "exchange." Much to our satisfaction, the enterprising Publishers not only forwarded us their Journal, but wrote a kind and encouraging letter, requesting us to reciprocate the courtesy. But this was not all, in a short time we received a large package of valuable books from them, with a promise that others should be sent from time to time, as they were published. In our letter, thanking them for these favors, we made some enquiries as to the most expeditious and certain means of effecting exchanges with foreign journals, all of which were kindly and satisfactorily answered. In fact, from these gentlemen, as well as from Editors and Publishers generally, we have received the utmost courtesy and consideration; and we hereby beg leave to return those who have thus honored us, our most grateful acknowledgments. But the whole story is not told. The picture has yet to be shaded before its completion. We have to record an instance of illiberality which deserves exposure, and which appears to us, all the more contemptible from its striking contrast with the generosity recorded above. Anxious to make some arrangement by which foreign exchanges might be affected, we wrote to H. Bailliere,

of New York, requesting to be granted the same privileges in this particular as are accorded to all other journals of respectability in this country. This individual is the agent for many foreign periodicals. and has it in his power to enter into just such arrangements as those proposed by ourselves. Apprehending, however, the loss of his commissions on a few dollars, he peremptorily declined to place the Medical Journal of North Carolina upon the same footing with other publications of a similar character, by refusing to comply with our most reasonable request. With all possible politeness, he regretted his inability to gratify us, and thus gave another and most significant illustration of that hollow-heartedness which is the only distinguishing feature of French character. With this statement of the case, we leave the whole matter to the public, convinced that it will readily distinguish between the genuine metal and the base alloy, and bestow its confidence accordingly.

TO POSTMASTERS.

It occasionally happens that copies of the Journal are sent by mistake to the wrong office, and we will thank Postmasters, in such cases, either to return them to us immediately, or to acquaint us with the fact. We will thus be saved great confusion, as well as the loss of the numbers themselves. Should it happen that a number is *refused*—which possibly may occur, as our assaults upon quackery are any thing but agreeable to the poor devils who live upon its abominations—we will be particularly obliged by an immediate report of the occurrence, so that we may have the satisfaction of disgracing the culprit by publishing his name upon a SEPARATE and prominent page of the Journal. We have sent copies of the Journal to all respectable physicians whose address could be obtained, with the hope of inducing them to become subscribers, but desirous at the same time that they should refuse to patronize our enterprise, by returning the number received, or

writing a civil note in reply, if they are not pleased with the work. But to have the Journal refused at the post office, declined as a bore, or rejected as a nuisance, by those for whose benefit it is intended, is a provocation we do not court, and of which we wish to be immediately informed, so that we may give these illustrious lilliputians the notoriety their littleness deserves, by publishing their names to the world.

OBLIGATIONS.

We are under obligations to Dr. Gibbes, for the "4th Annual Report to the Legislature of South Carolina, relating to the Registration of Births, Deaths, and Marriages, for the year ending December 31st, 1857;" to Dr. Logan, of Sacramento, for the "Transactions of the Third Session of the Medical Society of the State of California;" and to Dr. Martyn Paine, of New York, for his "Medical and Physiological Commentaries," and his admirable work on the "Institutes of Medicine."

All of these will be carefully read, and diligently reviewed, so soon as we can find time to devote to them.

VERATRUM VIRIDE.

We have been much gratified to find our views respecting this *wonderful remedy* endorsed by some of the most intelligent and experienced practitioners of this State. This is particularly grateful—because we feared that we should be compelled to stand alone in our opposition to its claims upon the confidence of the profession, and that our temerity in thus assailing a popular prejudice would involve us in some bitter and protracted controversy. Desirous of advocating the cause of truth, and influenced solely by an ardent love for the profession, we have spoken out our sentiments freely and fearlessly, prepared for any emergency, and firmly convinced that we have done our whole

duty in the premises. We have entered our formal protest against a remedy that we deem of doubtful expediency under the most favorable circumstances, and extremely dangerous in every instance. We have rejected both the *dicta* of the enthusiastic and the commendations of those interested *pecuniarily* in its employment. We have labored to examine the *rationale* of its therapeutical action, as a means of demonstrating its inadequacy as a remedial agent, and of rebuking those who would substitute it in certain affections for a more certain and efficient system of medication. Of course, it is far from agreeable to stand alone against the flood-tide by which this drug has been hurried on to its present popularity and importance; and it gives us infinite pleasure to be sustained heartily by one better able to meet the shock and to stand up against its fury. From the succeeding letter it will be seen that Dr. W. T. Howard, one of the most talented, extensive and successful practitioners in Carolina, uses the following language, in a letter to the editor, which we cannot refrain from quoting in this connection, though very complimentary to ourselves :

"I have read with pleasure your article on Veratrum Viride. I see that you have formed pretty much the same estimate of this vaunted remedy, of 'bloated promise and lank performance,' that I have. I have used it for nearly six years. My first seven cases—all of pneumonia—in which it was used, *all* terminated fatally! It is true, all were very bad, nearly desperate cases, and I do not now think that more than one, perhaps, could have been saved by a more judicious treatment. I have come to use the Veratrum Viride in only certain cases. Every one has met with cases of pneumonia in which the frequency of the pulse is excessive. In those cases, I give this agent in small doses, say 3 or 4 drops, along with other remedies, simply to reduce somewhat the great frequency of the pulse. This, I think, will finally come to be the estimate in which it will be held by prudent and judicious practitioners, and so used, I cannot doubt its value.

Last May, I had a case of typhoid fever immediately following parturition, in which a large amount of blood had been lost. I saw the patient, a young mulatto about 19, in the 4th week of her illness, with severe capillary bronchite, a respiration 48 or 50 per min., and a pulse varying between 156 and 160. The bowels were excessively irritable. The veratrum soon brought down the pulse to 114; and finally with blisters, and support, she recovered. I agree with you in nearly all that you say of this remedy; and can but think that the sterling good sense, and very handsome style in which you have presented it to the profession, will commend your article to the general approbation of discreet and reflecting minds. You may anticipate seeing it copied into other Journals, with the approval of the calm and judicious, but with no little opposition from the intemperate and *dashing* practitioners of the day."

A SUGGESTION.

A friend suggests the propriety of republishing all the addresses which have been delivered before the State Medical Society, as a means of preserving them in a more permanent form, as well as of exhibiting to the world the talents, the erudition, and the professional knowledge of the members of this flourishing society. If we do not adopt this suggestion in *toto*, we will at least reprint some of the addresses, as we think them calculated to do the state no little service.

COURAGEOUS CONDUCT OF MILITARY MEDICAL MEN IN INDIA.

We have much pleasure in making the following extract from a despatch of General Whitlock, commanding the Sangor Field Division, to the Resident at Hyderabad: "On the 19th inst., (April) in driving the enemy out of a very strong position covering the road to Bandah, I have the gratification of bringing under

your notice the conspicuous gallantry displayed by the officers and squadron of the Hyderabad Contingent, under Captain M'Intyre, in a charge the irregular calvary made on the enemy's guns under a most galling fire of grape and musketry, and in face of a most determined body of horsemen, who fought most boldly. . . . To Captain M'Intyre, Lieutenant Ryan, and Surgeon Bradley, I am much indebted for their gallant conduct on this occasion ; Surgeon Bradley attended his wounded under a very heavy fire."

DETECTION OF MINERAL ADULTERATION IN FLOUR BY CHLOROFORM.

A chemist at Charleville has contrived the following procedure, by which, according to M. Lassaigne, a ten-thousandth portion of mineral matter may be detected in flour adulterated with the same. A glass tube, 3 centimetres in diameter, and from 15 to 20 in length, closed at one end, is to be tightly corked at the other, so that fluid may be well shaken in it. From 1 to 3 drachms of the flour are to be introduced into this tube, which is next to be nearly filled with chloroform. The tube is now to be corked, well shaken, and left awhile at rest, until the separation has had time to take place. The flour rises to the surface of the chloroform, while the mineral adulteration falls to the bottom, and may be submitted to analysis in order to detect its exact nature.—*Moniteur des Hop.*

"UNIVERSAL DOUCHE."

Dr. A. M. Adams, upon a late visit to the Continent, speaks of an ingenious bath, devised by Dr. Molewater, of the Rotterdam Hospital, and terms it the "Universal Douche." The patient stands in a hollow niche in the wall, and parallel with his spine rises a central tube perforated with many apertures; while this tube is crossed at right angles by upwards of a dozen semi-circular tubes, similarly perforated, having thin concave surfaces towards the bather. A powerful stream of water is then turned on, and from all these perforated tubes, as well as from others below, the patient is enveloped in a perfect whirlpool of water, which lashes against his body in every direction. This form of bath is found to possess immense tonic influence in various atonic and hysterical complaints.—*Edinburgh Med. Jour.*

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ORIGINAL COMMUNICATIONS AND TRANSLATIONS.

A REVIEW OF DR. OTIS FREDERIC MANSON'S ESSAY ON MALARIAL PNEUMONIA.

By W. T. Howard, M.D., of Warrentown N. C.,

Presented to the State Medical Society of N. C., May, 1858.

(Continued.)

Dr. Manson states that "the *respiration* in the *chill* is more *frequent*, and the *dyspnœa* more *urgent* than in the *exacerbation*." The peculiar condition above noticed, in which a "coolness of the extreme parts" is said to be "daily," and "almost invariably observed during the morning hours," and in which there are great complaints "of *excessive internal heat*," is the one we suppose, to which reference is here made. This is the "*insensible chill*," and is followed by "a marked increase of fever." As we do not remember to have witnessed *this* particular condition—*this* "insensible chill," during the march of "malarial pneumonia," we can neither affirm nor deny the correctness of the above statement in regard to the "respiration" and the "dyspnœa," at that precise period in the progress of the disease. We suspect, however, that the peculiar state, designated by Dr. Manson as the "insensible chill," is, in a great many instances, at least, but the simple remission of the fever. According to our observations, after several paroxysms of remittent fever have occurred—usually from three to five—it is excessively rare for the febrile exacerbations to be preceded by a *chill*, or even by rigors; and such, also,

is the experience of our highest authorities. (*a*) When pneumonia is complicated by the phenomena of this fever—*remittent pneumonia*, as Grisolle calls it—the same law asserts its sway; and although the first few exacerbations are generally preceded by sensations of chilliness—by shiverings—yet, as the same observer truly remarks, they are *altogether absent in a great many cases.* In some instances, as in the ordinary forms of periodic fever, intermittent and remittent, the evidences of a chill may not exceed a certain amount of blueness or purplish lividity about the nails, and a marked coolness of the hands, feet, nose and ears.

During the usual *chill proper*, in both varieties of this fever just mentioned, the respiration is generally a little irregular and somewhat hurried, but in the succeeding exacerbation of the fever, it becomes regular and still more accelerated. This is observed to be the case particularly in young children. In almost all febrile diseases, if we mistake not, and more especially in inflammatory affections of the lungs—the respiration is generally most frequent during the intensity of the fever when the pulse is most accelerated, although the *normal* relation between it and the pulse is not always—yea, may not usually be—observed. We have already seen how strikingly perverted the pulse-respiration ratio often is in pneumonia—the number of the respiratory acts being out of all regular proportion to the increased frequency of the hearts pulsations; and what valuable aid this perversion of a physiological law frequently lends to diagnosis. We have often first suspected the existence of a latent malarial element in pneumonia by observing, so to speak, a peculiar sharpness and distinctness of the febrile exacerbations, during which the respiration was greatly accelerated and the dyspnœa excessive; and by noticing how obviously these symptoms were moderated as the fever subsided, when the patients have strikingly reminded us of those who having made great physical efforts to sustain a heavy burden, were quietly reposing in comfort, after the fatigues of the exertion had passed away. And, on the other hand, we bear in vivid remembrance other cases, one of the most remarkable of

(*a*) Bolling, A. J. Med. Sci., Vol. XI, 293; Wood, Op. Cit., Vol. I, 268; Chapman Benedict's Comp., 43; Stewardson, note to Eliotson's Prac., 338; Currie, On Remit. Fev., 46 (quoted by Bartlett); Morehead Op. Cit., Vol. I, 96.

which occurred in a daughter of my friend and partner, Dr. Charles Skinner, in which, after the rational symptoms of pneumonia had nearly disappeared, the patients were overtaken by an alarming state of depression, almost amounting to collapse, during which the surface presented an icy coldness, and exhaled a watery and exhausting perspiration, accompanied by a slow, irregular, sighing respiration, a filiform and frequent pulse, whilst the calm and impassive countenance beamed with the tranquil expression of one in a mild ecstacy. As the wilted and frigid surface slowly reddened beneath the quick and repeated application of powerful revulsives, and the prompt administration of restorative cordials, and returning warmth and a more encouraging pulse gladdened the hearts alike of physician and attendants, no doubt remained as to the cause and character of the sudden and overpowering prostration of the vital forces. The patient had been in the embrace of a terrible *chill*.

It may be urged that during the period of chill, the lung undergoes increased congestion, and that, *a priori*, the respiration must be more accelerated at that time. But this argument assumes that the congestion abates during the succeeding exacerbation, and, therefore, begs the question. We allege that, as a matter of experience, the local lesion progresses during the febrile exacerbation, and the pectoral symptoms increase in intensity. In this we are sustained by others. Wood states that "there is no stimulant so powerful as the paroxysm itself, and each recurring one has a strong tendency to sustain the inflammation. Indeed, the *pneumonic symptoms* are almost always *worse* in the *exacerbation of the fever*, and decline as that remits." (*a*) Drake says that when the pulmonary affection "consists of well developed inflammation, that condition does not, it is true, cease with the periodical abatement or cessation of the fever, but is greatly moderated to undergo *revival with the fever*; but when the inflammation is less intense, and the pulmonary lesion consists chiefly in passive congestion, it may so far disappear in the more or less perfect apyrexia, that the term intermittent becomes almost as applicable to the local as the constitutional affection." (*b*) Grisolle admits that

(*a*) Op. Cit., Vol. II, 28.
(*b*) Op. Cit.,, 869.

when the pneumonia has not passed the first stage, the local symptoms may—as an event rare and purely exceptional—disappear during the intermission, but it is during the *febrile exacerbations* that they are most developed; the pulse then becomes more frequent, "*respiration* is *accelerated*," &c. (*a*)

Dr. La Roche, also, gives similar testimony in regard to the abatement of the pulmonary inflammation, with the subsidence of the hot stage or exacerbation, "to recur again with the *accession of the fever*." (*b*)

Restrained, therefore, by the results of our own experience from rigidly verifying the entire fidelity of the picture which Dr. Manson has drawn of the phenomena that mark the period of chill, and the paroxysmal exacerbation of the febrile movement, in "malarial pneumonia," and while we cannot help suspecting—if we may say so with equal courtesy and frankness—that the habitual observance of some of those phenomena is somewhat confined, if not peculiar, to the sphere of his labors, or that others might interpret them very differently, yet it is very true that the fever is distinguished by a decided remittent and periodical character, and that it is very generally observed in that form of the disease.

The *Pulse*.—Dr. Manson states that in "malarial pneumonia," the "term *compressible* applies to the usual character of the pulse," and that it is "frequent, soft, of moderate fullness, and never or rarely very full, tense or hard." "In some few cases," it "has been observed to be nearly *normal*, or *morbidly* slow."

In their description of the pulse in acute sthenic pneumonia, authors do not uniformly employ the same language. Thus, Dr. Wood says it is usually full, strong, and only moderately accelerated, but when very frequent is apt to be smaller and less vigorous. Swett states that it is generally full and soft, and ranges from 90 to 110 in a minute. Andral remarks that it is very variable; but its most usual character is that of being frequent and large. Valleix represents it as generally full, accelerated and slightly compressible. Walshe alleges that unless when very frequent—and it may reach 140 or 160 beats per minute—it is

(*a*) Op. Cit., 432.
(*b*) Op. Cit., 452.

habitually full and resisting; is of much greater frequency in the mass of serious and fatal cases than in those of recovery, generally attaining its maximum frequency by the third or fourth day; but he has known pneumonia run its course, both in old persons and in young adults, with a pulse never exceeding 60. According to our own observations, the pneumonic pulse is scarcely ever hard and corded as it is in pleurisy, but is generally fuller and softer. The condition of the pulse depends upon the character of the disease—whether this be of sthenic or asthenic type. If malaria complicates a case of the former, it is habitually full and resisting, demanding those repeated bleedings which the older physicians practised previous to administering cinchona. Cleghorn, as we shall presently see, was a bold bleeder in the epidemic pneumonias which he encountered in the Island of Minorca, and of which he has left us so vivid an account. At the present time, nearly all diseases wear the livery of enervated power, and in various localities far removed from each other, pneumonia, as well as other affections, appears divested to a marked extent, of its former dominant sthenic character (*a*.) Everywhere this seems to be the prevailing medical constitution of the times, of which the reign of carbuncles, carbuncular boils, whitlows, etc., that for the past few years have prevailed in such unwonted proportion, may be considered among other evidences, as a striking proof. A recent writer states (*b*), that he is as cautious in ordering a patient to be bled as a surgeon would be in deciding upon the amputation of a limb; and having been for the last three years connected with a dispensary, where all the usual forms of disease present themselves, he had recommended bleeding only once during the whole of that period in that institution. (*c*)

(*a*) Todd, Ranking's Abs., No. xv., 227; Risdon Bennet, Ib.. No. xvii., 74; Routh, Ib. No. xxi., 52 ; Fincham, Ib., No. xxvii., 39, and Cameron, Ib.. 40 et seq.; Ramsay, *Char. Med. Jour.*, Vol. vi., 22 ; Perry. Ib., Vol. xii., 178 ; Ames, *N. O. Med. and Sur. Jour.* Vol. x.. 424; *N. O. Med. News and Hos. Gaz.*, Vol iv., 439 ; Habershon, A. J. Med. Sci., Vol. xxxv, 514 ; Barlow, Op. Cit.. 202; Watson, Op. Cit., 574.

(*b*) Semple, A. J. M. Sci. Vol. XXXV, N. S., 513.

(*c*) During a sojourn of some weeks at the Virginia Springs, during the past summer, we conversed with physicians from different portions of the country in regard to bleeding in inflammatory diseases, and especially in pneumonia, and did not meet with one who had not been obliged to discard the lancet altogether in the vast majority of cases, or to use it very sparingly in those instances admitting of its employment at all. Even among those living in the mountains, such was the experience of all with whom we had any conversation. There never was a greater change in the therapeutics of inflammatory affections than the past few years have evinced.

It follows, then, that a soft and compressible pulse cannot be deemed, at the present time, at all peculiar to that form of pneumonia which is imbued with the poison of malaria; and in that variety of the disease termed *typhoid*, from the predominance of the asthenic state—it is habitually soft and readily compressible.

V. *Delirium.* After describing the character of the recurring febrile paroxysms as already quoted, Dr. Manson adds:

"A new symptom now sets in. The patient, who before may have been entirely rational, or only slightly incoherent in sleep, or at the height of the exacerbation, suddenly becomes wildly and persistently delirious. * * * * The delirium increases in intensity, the patient growing almost uncontrollable, and requiring force to keep him in bed; and when not watched rushing out of the house when nearly pulseless and cold. Quiet only succeeds to this raving to be quickly succeeded by the curtain falling on this last scene in the agonizing drama."

Under the head of "Diagnosis, between malarial and other forms of pneumonia," Dr. Manson continues the consideration of this symptom:

"The presence of Delirium is more common in the malarial than in the simple form. In the former it is usually distinctly intermittent or remittent in the early stages. At that period it also is usually mild in its character, the patient seeming to be in a state between sleep and awake, muttering incoherent and rapidly changing fancies, but when fully aroused and his attention directed, replying in a rational manner. The delirium is evidently of a neuralgic character, being often associated with a cool skin and feeble pulse, with no other symptom belonging to organic cerebral lesion, and like the hallucinations of delirium tremens, to which it bears many and striking resemblances, is relieved by sleep, to recur again, however, in the febrile paroxysm. Delirium, however, supervenes under other circumstances, which, although we have already noticed, constitute such an important feature as will excuse a repetition. Often, without any previous cerebral symptoms whatever, after the pneumonic symptoms have been mitigated or have entirely disappeared, and confident hopes are entertained of the patients recovery, a furious delirium suddenly occurs. I cannot refrain from adding this striking characteristic as drawn by Cleghorn. 'Besides some abatement of the fever which happened every morning, it was remarked that upon the third day or begining of the fourth there was frequently a great remission, sometimes a total cessation of every violent symptom, so that the sick were thought to be out of danger, but on the fourth or fifth a delirium suddenly came on, or the breathing became more difficult than ever, and one or both of these symptoms increasing hourly, the patient expired either suffocated or raving mad, unless nature or art assisting, he had the good fortune to escape by some critical evacuation.' It has happened to the writer to have seen cases in which every *rational and physical* sign of convalescence had made their appearance, but who on the next visit were found to be in the same condition as when first attacked, without any known cause which could be possibly assigned for the change. In these cases it will be perceived that the malady was strictly intermittent, and the recurrence of the symptoms was evidently connected with the periodical character of the affection, and the liability of that class of diseases to relapse."

1. *Frequency* of occurrence—the *time* at which it appears, and the *character* of the *delirium*. It will be seen that *delirium* is admitted to occur in simple, frank pneumonia; but our author alleges that it is "*more common*" in the "malarial" form. Yet he has furnished no proof whatever of his assertion. This point can only be determined with precision by an extensive series of accurate statistical researches. Certain it is, that delirium occurs sufficiently often in the course of simple pneumonia to be prominently noticed in the writings of many eminent observers. (*a.*) Out of thirty *fatal* cases of common pneumonia, from whom exact statements were obtained in regard to this symptom, Louis found that eight, or about a *fourth* part had more or less serious delirium at a period and during a space of time that varied much. In one case of seven days duration, it commenced with the disease, and continued uninterruptedly until death; quiet during the first five days, but afterwards so noisy and violent as to require the straight jacket. In five patients who died between the *eleventh* and *twenty-fifth* days it began from *four* to *seven* days before death, and ceased only with life. Out of fifty-six pneumonic patients that *recovered, ten* had delirium, three during twenty-four hours only, others during a space of time of from two to six days. It commenced with the disease in one patient, and on the second day in two others; between the *fifth* and *fifteenth* in the remainder of the patients. In three the agitation was so considerable as to necessitate the use of the straight jacket. It thus appears that *delirium* was present in *eighteen* of *eighty-six* cases—a little more than *one-fifth* (*b.*) In *sixty-five* cases observed by Andral, *fifteen* offered *delirium* more or less pronounced. In 286 cases in which the state of the cerebral functions was sufficiently studied, Grisolle observed that *twenty-seven* presented at certain periods of the affection, *delirium* more or less intense. Of the cases treated by Briquet at l' Hôpital, Cochin delirium was present in about *one-seventh*. According M. M. Hourmann and Dechambre, it is rare for pneumonia to run its course among the

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(*a*) Gerhard, Op. Cit., 144; Swett, Op. Cit., 105-6; Watson, Op. Cit., 567; Wood, Op Cit., Vol. ii., 11; Barlow, Op. Cit., 188; C. J. B. Williams, Op. Cit., 610; Drake, Op. Cit., 855, etc.

(*b*) Op. Cit., Vol. ii., 143-146-147.

old women at Salpétrière without derangement of the intellectual faculties, and *delirium* is an habitual accompaniment. Uniting statistics of recent date, Grisolle found *delirium* in 54 of 435 cases, or about *one-eighth*. He insists upon the importance of studying the import of this symptom, not only because it may arise from different conditions offering opposite indications, but also because it may in some cases give rise to the belief in the existence of a cerebral disease, when in fact the affection is seated in the lungs. And the necessity of a complete examination of the organs is shown by the fact, that when this has been neglected, it has not uncommonly happened that persons have been carried to lunatic establishments on account of a temporary mania, developed during the acute stage, or *the resolution of pneumonia*. Thus, M. M. Thore et Aubanel, of Marsailles, report that they have seen many times persons arrive at l' Hospice de Bicêtre, who were sent there as insane, and who were only affected with a passing mania developed, as Grisolle says, during the acute period, or *during the resolution of pneumonia*, and which was dissipated very rapidly. Delirium is more common in fatal cases, and in double pneumonia—being observed in *one-fourth* of either class; and is more frequent in men than in women in the proportion of *twenty-one* to *six*. We find that according to Huss, the latter statement is also true to a certain extent in regard to typhus and typhoid fever; as, of 182 men 87, or 47.8 per cent., and of 68 women 23 only, or 33.8 per cent., became delirious (*a*.)

The *time* at which delirium manifests itself in the course of pneumonia varies very much, as we have just seen from the recorded experience of Louis. According to Grisolle's observations, it generally appears from the *fourth* to the *sixteenth* day; and much sooner in drunkards than in others. Thus, whilst the former commenced to be delirious towards the *fifth* day, the latter presented this symptom towards the *tenth*. It offers, also, different characters in the sober and in drunkards; but in both may assume various forms, from slight incoherence to furious delirium.

(*a*) Statistics and Treatment of Typhus and Typhoid Fever, from twelve years experience (3,186 cases), at the Seraphim Hospital in Stockholm, by Magnus Huss, M. D., 61.

Dr. Manson states that in "malarial pneumonia," the *delirium* "is usually distinctly intermittent or remittent in the early stages," and is "usually mild in its character, the patient seeming to be in a state between sleep and awake, muttering incoherent, and rapidly changing fancies, but when fully aroused and his attention directed, replying in a rational manner." But every one must have observed that delirium is "usually" greater during the intensity of the fever, and oftener at night, when only it may exist, in all febrile affections in which it is a prominent symptom; and in this sense it may be said to be intermittent or remittent," just as it is when present during the paroxysmal exacerbations of the fever in "malarial pneumonia." And in regard to the *character* of the delirium, it may be stated, that Dr. Manson's description agrees pretty well with that given by Grisolle when it occurred in the sober. In this class of cases, he found that it was characterized only by a little incoherence; the patients talked alone, and replied to the questions addressed to them in an unconnected manner. Sometimes they gave sensible replies, but this was only momentary. Three of this class had great agitation; they spoke loudly, made great efforts to rise, and were obliged to be confined to bed by the jacket. The delirium lasted from one to eighteen days.

Dr. Manson thinks that, as delirium may "supervene" late in the career of "malarial pneumonia"—"often without any previous cerebral symptoms whatever, after the pneumonic symptoms have been mitigated or have entirely disappeared, and confident hopes are entertained of the patient's recovery"—it exhibits, in this respect something differentially diagnostic of the malarial form of the disease. Under these circumstances, it is said to be "furious," and to "occur suddenly;" "the patient grows uncontrollable, and requires force to keep him in bed; and when not watched, rushes out of the house when nearly pulseless and cold. Quiet only succeeds to this raving to be quickly followed by death." But we have already seen that in a certan number of Louis' cases of simple pneumonia, as well in those who died, as in those that recovered, delirium first declared itself during the last days or hours of the disease, and in some it was so violent as to demand the use of the straight jacket. Grisolle, also, was com-

pelled to confine some of his patients to bed by the same means. Indeed, he distinctly states that, "when the progress of pneumonia has been rapid, it may not exhibit itself *until the decline of the disease, or during convalescence.*" Finally, in order to prove how little importance can be attached to the *particular time* at which delirium first makes its appearance, as diagnostic of either form of the disease, and as a complete offset to Dr. Manson's confident statements, it is only necessary to add that, Dr. Morehead, in his recent elaborate work on the diseases of India, to which we have already had occasion several times to refer, actually places the supervention of delirium "*when the pneumonia is not far advanced,*" among the most important differential signs of that form which is complicated by a malarial impression. (*a*) Now, which of these observers, both of whom profess to speak from *experience*, holds out the true light that will safely "guide the neophyte through the labyrinth before him"? One or the other of these lights must be an *ignus fatuus*, and will often lead into a bog. Doubtless both are sometimes correct; one observer has oftener met with this phenomenon in the first few days of the affection, and the other at a later period; which proves that there is no uniform rule on the subject worthy of confidence, and that diagnosis, therefore, derives no benefit from it whatever.

We cannot admit, then, that delirium, either by the *frequency* of its occurrence, the *time* at which it appears, or by its *character*, offers any special indication of pneumonia imbued by the poison of malaria.

2. *Nature* and *Cause* of the Delirium—Dr. Manson speaks of *delirium* as a "symptom *belonging* to *organic* cerebral lesion;" and yet affirms at the same time that it "is *evidently* of a *neuralgic character*, being often associated with a cool skin and feeble pulse"! We must confess that, to us at least, this is not very clear. We do not readily understand how *delirium* can be "*neuralgic;*" but if by this Dr. Manson means that it is often dependent upon some functional derangement of innervation, the nature of which is unknown, but which is unconnected with any appreciable lesion of brain structure or its membranes, we fully agree with him. And from its being spoken of as "*often* associated with a *cool*

(*a*) Op. Cit., Vol. II, 350.

skin and *feeble pulse,*" one might very naturally infer that the idea of its being disconnected with active *inflammation* was intended to be conveyed; but we are told in the next line that it is a "symptom *belonging* to *organic* cerebral *lesion*"! Again, he is referring to delirium "in the early stages" when it is said to be "associated with a cool skin and feeble pulse," at which time it is described as being "intermittent or remittent—that is occurring "at the height of the exacerbation," and subsiding during the remission. But if it manifests itself "at the *height* of the exacerbation—during the *acmé* of the "*febrile* paroxysm"—is it *then* "associated with a *cool* skin and *feeble* pulse"? One would suppose not, certainly; but if not, why assign these conditions, which are then usually wanting—do not exist—as a prominent part of the evidence that is adduced to show that the delirium is of a "*neuralgic* character"? So far as they are concerned, they prove (according to this logic, as it seems to us) just the opposite; for Dr. Manson will hardly contend that the "*height* of the *exacerbation*" of the *fever* is the period when the "*skin*" is "*cool*," and the "*pulse*" "*feeble*." But let us now reverse the picture, and suppose that delirium occurs—as Dr. Manson has represented it generally to do—at "the *height*" of the *febrile exacerbation*, and during "the early stages," when the "skin" is *not* "cool," and the "pulse" *not* "feeble." Are we to understand that a hot "skin" and a "pulse" that cannot be called "feeble," indicate that delirium is *not* of a "neuralgic" (sympathetic?) nature—that when it occurs under these circumstances, it necessarily betokens mischief within the encephalon?* If so, this is most dangerous doctrine, and utterly at war with sound pathology. Nevertheless, it would seem that Dr. Manson does mean to inculcate such a doctrine,

*As we are anxious not to misconstrue, or even unintentionally misrepresent Dr. Manson's views, the reader will please refer again to all that he says about delerium (see p. 410). When he speaks of "a cool skin and feeble pulse" as evidence that the delirium is of a *neuralgic* "character," he, also, adds—"with *no other* symptom belonging to organic cerebral lesion." This would seem to imply that if there were "*other*" symptoms of "organic cerebral lesion" present, the conditions of the skin and pulse assigned, might not be sufficient to indicate that the delirium is of a "*neuralgic* character. If, however, the "skin" be hot, and the "pulse" *not* "feeble" and still there be "no other symptom of organic cerebral lesion" existing, he leaves it to *inference* as to what the "character" of the delirium would then be. But no doubt whatever can arise in regard to the fact, that delirium is represented as a "symptom *belonging to organic* cerebral lesion;" for on this point, the language is positive and unmistakeable.

when he speaks of delirium as a "symptom *belonging* to *organic* cerebral *lesion*"! With all due respect for our talented author, we must be permitted to express our very great surprise that so zealous a student of modern medical literature should have fallen into so grave an error as to have represented delirium,—which is so common in various acute febrile disorders, and occurs in certain constitutions whenever fever exists from almost any cause whatever,—as a "*symptom belonging to organic cerebral lesion*"!! In his classical work on "General Pathology," M. Chomel assures us that, "delirium will occur *twenty times*, perhaps, as a *sympathetic*, where it does *once* as an *idiopathic* phenomenon, that is, one dependent upon disease of the brain itself, at least in acute affections." (*a*) And Todd, in his beautiful lectures on affections of the nervous system, observes—"delirium does not necessarily connect itself with any actual disorganization of the brain or its membranes, nor does coma either; but both these formidable states may take place in a brain that shall reveal, on the minutest scrutiny, no appreciable aberration from the natural standard." (*b*) Grisolle found the anatomical characters of recent meningitis in nearly *one-third* of the fatal cases of pneumonia attended by delirium; but in the remaining *two-thirds* the state of the brain and membranes in no wise accounted for the functional disorder. Seven years after the publication of his great work on pneumonia, he found the signs of recent meningitis in only about a *fifth* of the cases. Louis, in his autopsies of typhoid fever patients, examined carefully the state of the brain in twelve cases that had little or no delirium, and in twelve others who died with violent delirium. Among those who had no delirium, there were four cases that presented some redness of the cortical substance of the brain; in one, inflammation of the optic thalamus; in one, slight softening of the brain; and in the remaining six, the cerebral substance was quite healthy. In the cases attended by violent delirium, there were five in which the morbid appearances were slight, there being some redness and vascularity of the cortical substance, but no more; in one, livid injection of the brain and its membranes; in the remaining five,

(*a*) Op. Cit., 324.
(*b*) Clinical Lectures on Paralysis, Disease of the brain &c., by Robert Bentley Todd, M. D., 119.

the substance of the brain was of a natural color and consistence, and appeared to be quite healthy. In commenting on these two sets of cases, Dr. Stokes remarks—"You will observe that the difference in the morbid appearances is extremely slight, and that presence or absence of delirium seems to have very little connection with the perceptible state of the brain. * * * The conclusion, then, which we must come to, is that inflammation of the brain does not necessarily imply delirium, nor does delirium imply any certain disease of the brain." (*a*) The results of Louis' researches have been amply verified by the observations of others. Thus, Flint in his excellent Reports on Continued Fever, in answer to the inquiry, "Upon what pathological condition or conditions is delirium in continued fever dependent?" replies, "Observations have abundantly established, that it is *not* due to *inflammation*, and hence, the occurrence of encephalitis, as a complication of the fever, is not to be predicated upon *this* symptom, *however prominent it may be.* This is an important *practical fact.*" (*b*) It appears to us, therefore, to be very "evident"—indeed to be beyond all cavil—that delirium is *not* a "symptom *belonging* to *organic cerebral lesion*," and that it was *faire un faux pas* on the part of Dr. Manson so to represent it; for it is one of those grave mistakes that would inevitably lead to no small errors in practice, should some "novice" be thereby led to act upon it in the treatment of pneumonia, as well as in various other diseases.

There are certain circumstances that seem to be connected with the *cause* of delirium in some cases of pneumonia. Thus, we have seen that it generally accompanies the disease in the *aged*; and *intemperance* must also be regarded as one of the causes, since in the pneumonias of *one third* of those addicted to drink it occurs, and generally comes on from the *fifth* to the *seventh* day. It is well known that Louis thinks that the frequency and violence of this symptom in acute affections are in proportion to the febrile excitement. (*c*) Grisolle considers the idiosyncracies of individuals, their habits and moral condition at the time of seizure, as exercising an important influence in its evolution; in-

(*a*) Lec. on the Prac. of Physic, by Wm. Stokes, M. D., second American Ed., 466.
(*b*) Clinical Reports on continued fever, based on Analyses of 164 cases, etc. By Austin Flint, M.D., 155.
(*c*) Op. Cit., Vol. ii., 150.

clines to refer it to infection of the fluids in certain typhoid and malignant pneumonias, and believes that very often no satisfactory explanation of its existence can be assigned. (*a*) Of course, in those instances in which meningitis supervenes, the delirium may be due to this cause.

3. We must now notice the quotation from Cleghorn, as Dr. Manson seems to attach special importance to his testimony. In order to appreciate it fairly and justly, we must understand fully the circumstances under which it was given, and the character of the subjects in which this symptom occurred.

This physician, while surgeon to the British troops stationed in the Island of Minorca, from the year 1744 to 1749, had an extensive practice. He divides the diseases of that island, of which he gives an account, into two classes of anniversary epidemical fevers, which he calls the summer and winter fevers. The former break out in June or July, and cease about January; the latter appear about November, and are rarely seen after the summer solstice. All the diseases of that climate are termed by him *periodical*, having remissions at intervals more or less considerable. Pleurisy or pneumonia are regarded as a modification of marsh or periodic fever, occurring in winter; and fluxes and painful obstructions in the chylopoietic viscera, as the same fever prevailing in summer, the only difference being in the organs pathologically invaded, and the different atmospheric influences that produce them: the cold of winter and the heats of summer, being in turn the exciting causes. The inhabitants of the island are described as being well built, strong and active, and of sanguine temperament. Such is the natural impetuosity of their temper, that the slightest cause provokes them to anger; quarrels about the merest trifles daily break out among neighbors and relations; and family disputes are transmitted hereditarily from father to son, as they are equally incapable of forgiving and forgetting an injury. So that lawyers and pettifoggers are very numerous, yet there are still too few for the clients. The majority of their dishes are highly seasoned with the most stimulating spices; they eat a great deal of oil, using it not only with

(*a*) Op. Cit., 386 et seq.; and for reference to several authorities introduced under this head.

salads, but also with boiled and fried fish, greens, etc. People of all conditions consume wine at their meals, of which there are three or four plentiful ones a day; and they are so much addicted to the use of tobacco as never to be without a pipe, either in their mouths or their pockets. In summer almost every one sleeps an hour or two after dinner, and some follow this practice throughout the year; and a *fourth of their time is made up of holidays, sports and pastimes.*

Such were the luxurious and excitable people, as described by himself, among whom Cleghorn made his observations; and we do not wonder that their pneumonias were of the most rapid and violently inflammatory nature. "Indeed," says he, "such was their rapid progress, that if any of them survived the seventh day, it seemed to be *entirely owing to bleeding.*" At first, during the reign of those epidemical pleurisies—as he calls them, but which he says (and as his autopsies proved,) "ought rather to be termed peripneumonias"—their quick progress and uncommon mortality surprised him greatly, and he attempted to cure them by bleeding once or twice a day, as he had always done in inflammatory fevers; but before they were blooded two or three times, the exacerbation appeared on the fourth or fifth days, and defeated all attempts by bleeding, blistering or otherwise, to relieve them. Startled by the disastrous results of this practice, he reviewed the whole progress of the disease, its symptoms and issue, and then used the lancet with more caution, relying chiefly on the speedy application of blisters. This practice however, was less successful than the first, and he was convinced that instead of too much, too little blood had been lost. He now bled to 30 or 40 ounces within the three first days; bathed and blistered the legs, etc. This plan succeeded in several cases, and although for the most part the patients escaped, yet they recovered with difficulty, and continued coughing, spitting and sweating at night for weeks. At last, when the disease raged with the utmost fury, he laid the patient in a horizontal position, and bled till the pains abated, or he began to faint, neither of which happened before xvi., xx. or ℥xxx. were taken. This was done, if called to the patient in the morning; but if the symptoms continued, the same quantity was taken in the afternoon. The

next morning, if there was the least room to suspect that any obstruction remained in the head or breast, the bleeding was repeated; so that between 48 and 54 ounces were frequently taken away during the first 24 hours. This copious loss of blood was followed by an anti-phlogistic purge; but if the symptoms did not cease, or if the pains and difficulty of breathing returned, or if the symptoms betokened mischief about the brain, the bleeding was again repeated, taking away at different times xii., xviii. or ℥xxiv. in the space of a day, either by the lancet or cups or both, as occasion required. By such heroic means only, could Cleghorn succeed in saving his patients. (*a*)

Now, when we reflect upon the character of the subjects, the violence of the inflammation—which, as we shall hereafter see, passed into the third stage of modern authorities, "frequently even in those who died *so early as the fourth day*" (*b*)—and the very speedy termination in death if not promptly combatted by the copious loss of blood, can there be reason for surprise that *delirium*—which, as we have already seen, the best modern observers have proved to be not uncommon in the pneumonias of our day, and in which the grade of the inflammatory process is nothing like so vehement—should have been so prominent a symptom in the cases encountered by Cleghorn? According to Dr. Manson himself, the disease which he depicts is "*widely different* from that described by the majority of authors"—is "unlike in *character* and *nature*"—"the pathological condition of the lung partaking more of the character of *congestion*, than open *inflammatory* action." How much greater then, is the difference "in *character* and *nature*" between *his* "malarial pneumonia," and the vehemently inflammatory disease described by Cleghorn? And yet, in giving the finishing touch to the picture which he painted of his "malarial pneumonia," Dr. Manson could "not refrain from adding" a "*striking characteristic* as drawn by Cleghorn"! It appears from this that diseases, however much they differ in their essential nature, may present, notwithstanding, the very same "striking *characteristics*"! But if a given symptom

(*a*) Diseases of Minorca, etc. By Geo. Cleghorn, M. D. Fourth edition, London, 1779, Chap. vi.
(*b*) Op. Cit., 267.

be "characteristic" of several and very "widely different" conditions, it is not very clear how it can be *diagnostic* of either. So that when we come to look particularly into Cleghorn's testimony, the character of his patients, and the terrible ravages of the most rapidly destructive inflammation—"the pathological condition of the lungs" being so strikingly and "widely different from" the account which Dr. Manson gives of his own cases—we do not very readily perceive the appositeness of the quotation, or in what manner it strengthens his cause. Moreover, Dr. Manson tells us on the third page of his essay, that owing to "the progress of pathology, the advance of therapeutics, and still more, the change of that which Sydenham termed the Medical Constitution of the period," it is "*imperatively necessary*, not only for the attainment of correct views of the nature of disease, but *even for the recognition of its physiognomy*, that its *descriptions* should be often *revised*, if not *altogether re-written*." We imagine that the symptomatology of few diseases has undergone changes so great as those that distinguish the affection described by Cleghorn and Dr. Manson; and "the *pathological condition of the lungs*" is, surely, "*widely different*." It would seem, therefore, that in "revising and rewriting" its history, in order to recognize "its physiognomy," the "descriptions" should correspond with the great modifications in its "nature," for symptoms that were "strikingly characteristic" of the one, may be not at all distinctive of the other. Dr. Manson says not one word about the average *duration* of the disease in his experience; but it is certainly not by any means "strikingly characteristic" of any form of pneumonia that we have seen, for "a furious delirium suddenly to occur," and for "the patient" to "expire, either suffocated or raving mad," *on "the fourth or fifth day."*

4. Dr. Manson says:—

"It has happened to the writer to have seen cases in which every *rational and physical* sign of convalescence had made their appearance, but who on the next visit were found to be in the same condition as when first attacked, without any known cause which could be possibly assigned for the change. In these cases it will be perceived that the malady was strictly intermittent, and the recurrence of the symptoms was evidently connected with the periodical character of the affection and the liability of that class of diseases to relapse."

We are not told by Dr. Manson *how often* he has "happened" to see these cases, nor does he in any manner indicate the relative frequency of their occurrence. It is important to remember

however, that *relapses* occur in ordinary uncomplicated pneumonia. Copland says that "*relapses* of pneumonia are not infrequent during some period of convalescence" (*a*); and their frequency has been estimated by Briquet at about *one-fifth*, and by Grisolle at about *one twenty-eighth*, of the cases (*b*). Mere acute recrudescence, therefore does not by any means prove "the affection" to be of a "periodical character," if by that we are to consider as acting, the particular diathesis of periodic fever. But if we are to understand Dr. Manson, as stating that cases of pneumonia occur, which after yielding to treatment more or less kindly, are found suddenly to become worse, and that after a careful survey of their condition, and the attendant circumstances, no reasonable cause can be assigned for so great a change, save in the evolution of a new element which now for the first time presents itself, and that that element is the manifestation of the latent poison of malaria, we have already borne our humble testimony to the truth of the statement. We frequently see this condition of things developed during the career of various diseases in a malarious region. Many a patient dies unexpectedly in the first paroxysm or explosion of malarial poison, which strikes the sufferer in the earliest moments of relief from the perils that have so recently surrounded him, when like an over-burdened man he had just strength enough left to stagger up to the top of the hill. But we differ from Dr. Manson in his explanation of the phenomena. He thinks that the disease was from the very first of malarial origin, and that the sudden and unexpected change in the condition of the patient is "evidently connected with the periodical character of the affection and the liability of that class of diseases to relapse." It appears to us that a more correct solution of the case may be offered. Every physician practising in malarious regions must have observed that, cases of periodic fever not unfrequently occur when the system becomes depressed from any cause whatever—as from a bad compound fracture, parturition, and more especially when attended by great loss of blood. Dr. Manson, in another place, gives expression to this truth in the form of an "*axiom*," thus : "periodical fever is often

(*a*) Dict Prac. Med., Vol. II., 881.
(*b*) Walshe Dis. Heart and Lungs, 430.

suddenly developed *by any depressing cause*, such as a blow, a fall, a shock, and by the *incidental occurrence of other diseases.*" Just so; the patient having been exposed to the causes of pneumonia, gets an attack of that disease, his system is depressed by the treatment deemed necessary for the cure; and while thus enervated, the influence of the poison of malaria is for the first time developed, and acts with the greater violence on the prostrate powers of the sufferer. In the case of the sudden accident, or the parturient woman, no one would attribute the fracture, or the accouchment to a malarious impression; why then, do it in the case of pneumonia? Again, although periodic fevers are, as every one knows, very liable to relapse, yet they are prone in so doing to observe a septenary law, and to return on certain days. But in the instance before us, the patient is for the first time, and suddenly, over-taken by the action of the poison of malaria—is in the greatest danger from its violent impression, and of course is not in a state of convalescence. How then can it be said that he has suddenly fallen back in consequence of "the periodical character of the affection and the liability of that class of diseases to relapse," when the periodic fever element has only for the first time "made" its "appearance" in the case?

We have now reviewed, at great length, Dr. Manson's account of the leading symptoms, and dwelt particularly on those features that are considered distinctive, of "malarial pneumonia;" we have carefully compared it with ordinary simple pneumonia, as it is so faithfully described by many of the best modern writers on the subject; we have endeavored to seperate and define those symptoms that truly belong to the former, and attempted to show that others adduced by our author as diagnostic, do not, in fact, form an essential part of its history; and what is the conclusion to which we have been naturally led? Why that some of the leading phenomena of remittent fever are engrafted upon, and blended with, the rational symptoms of ordinary pneumonia; and that those symptoms thus united with and complicated by the tokens of an accidental affection, represent and portray the so-called "malarial pneumonia." We are not surprised, then, that when Dr. Manson comes to unfold his views of the "cause and nature of malarial pneumonia," he should be led to declare that

"the reader who has had frequent opportunities of observing remittent fever at the bedside, or those who have not, who have known the writer, (Dr. Manson,) by a perusal of his imperfect descriptions of that disease, cannot fail to perceive the close resemblance between the phenomena presented by that affection, and the disease under consideration; indeed, the *similitude is almost perfect, with the addition of the signs and symptoms of the thoracic lesion.*" *What* "thoracic lesion"*? Pneumonia?* Precisely: it is *true pneumonia* complicated by the incidental impression of a malarial law; and this readily explains the fact that Dr. Manson's "malarial pneumonia" offers so many points of natural and striking resemblance alike in its onset, march, leading phenomena, and termination, with the ordinary forms of pneumonia with which we have compared it. Where then is the necessity—what valid reasons are there—what good can result from erecting it into a separate and distinct variety of pneumonia, if by that we are to recognize, as does Dr. Manson, a disease pathologically "widely different from"—"unlike in character and nature"—the ordinary forms of pneumonia with which it is so closely allied as to be often, if not always identical, when we detach from them the well-known phenomena of a mere accidental complication, and that, too, without adducing one solitary *autopsy* to give some countenance to so bold an assertion, ventured without proof? Instead of his presenting positive and matter-of-fact evidence of that kind, in accordance with the requisitions and exacting spirit of modern medicine, we do not remember anywhere to have seen (what appears to us) a clearer example of the *petitio principii*, than our author has exhibited. But we shall return to this branch of the subject, when we come to discuss the "cause and nature of malarial pneumonia."

VI. We are now prepared to inquire whether we are so poor and destitute of reliable information respecting the malarial complications of pneumonia, as our author alleges. He says that:—

"The existence of a form of pneumonia, having some *undefined connection* with periodical fever, has long been known and noticed; but unfortunately for the student of the present day, the affection has not attracted the attention of recent investigators, and is only cursorily and imperfectly noticed by any of the modern standard authorities."

In his learned and phisolophical work on pneumonia, Dr. La

Roche states that the idea of a *close connection*, as regards both causation and nature, between thoracic inflammations and malarious fevers of various grades and types, has long been entertained, and continues even now to be advocated by writers of respectable standing on the other side of the Atlantic, and particularly in our Southern and Southwestern States, where there are many warm and uncompromising defenders of the doctrine. He warns the modern advocates of this belief, that they are treading on ground already and frequently travelled over and as frequently abandoned ; and that they must *sedulously avoid laying claims to originality*. Indeed, so extensively and pertinaciously have these doctrines been entertained in certain quarters, that Dr. La Roche's volume of 502 pages, was written mainly to disprove the necessary relation in causation and nature between pneumonia and periodical fevers, and to prove that although independant of each other, as regards nature and cause, they *combine together*, and form, like other complaints, hybrid diseases, which must not be considered as peculiar forms of either. Of the occurence of these latter cases, he tells us no experienced or well-read physician can be ignorant; for he must be fully aware, that in miasmatic regions, they are frequently encountered, and that there pneumonia, like, indeed, very many other diseases, often, if not generally assumes, to a greater or less extent the periodic type.

Dr. La Roche continues :—

" In many such cases the inflammation of the lungs presents itself in combination with symptoms appertaining to ordinary bilious remittent fever, or hepatic or bilious derangement, giving rise to what is denominated bilious pleurisy, a form of disease accurately described by many American and European writers. In other instances, the bilious symptoms are not so prominent, and the affection of the lungs is associated with those of simple remittent or intermittent fevers. Indeed, instances of remittent and even intermittent pneumonia, pleurisy and pulmonary catarrh, in which the inflammation is complicated with symptoms indicating the existence in greater or less purity of the element of periodicity, are to be found described in the writings of the most reliable authors. Morton, who early called attention to them, and indicated the treatment they required, had seen a hundred such cases, and since his day they have continued to be adduced as objects of familiar professional observation, not only in Europe but in this country also.(*a*) * * * Though sometimes encountered in ordinary fever seasons, and in most places subject to malarial fevers, and though more particularly rife in times of violent and

(*a*) In support of these positions, Dr. La Roche mentions a long array of authorities, the list of which occupies nearly half a page, printed in small type, of an ordinary octavo volume.

wide spreading epidemics, and in very sickly localities; this complication of inflammation of the lungs with miasmatic fever is not unfrequently found to occur under circumstances of a different kind; for, by virtue of the power of latency possessed by these, the system remains predisposed to them; and the same exciting cause which occasions an attack of pneumonia, long after or in a healthy season, calls into activity the malarial poison, and the result is the compound disease under consideration. This condition of things has been observed in all miasmatic countries, and nowhere more strikingly than in our own. * * Inflammation of the lungs, when it presents itself in cases characterized by symptoms of periodic or autumnal fevers, is to be viewed in no other light than as a complication, and not as part and parcel of the fever in question. Each disease may, and often does exist independently of the other; but both may, and sometimes do coexist in the same subject; and while in such cases the febrile disease is due to the specific poison, the pneumonia arises from the same cause that would under any other circumstances have produced it. In such instances of complication, each of the two diseases may be simply modified by, or as it were, tinged with the other, i. e., a violent pneumonia may present feeble symptoms of an endemic fever, or, *vice versa*, to a severe attack of fever may be associated symptoms of slight pneumonic inflammation. In some cases they start together. At other times the one precedes the other; the former acting by enhancing the susceptibility of the system to the action of the cause producing the second; or calling into play morbid conditions which before had lain dormant or latent in the system. To this it may be added, that while in some instances the pulmonary inflammation is subordinate to the fever, and, so long as it has not reached beyond the first stage, disappears during the apyrexia of the intermittent form, to recur again with the accession of the fever; and while it very greatly abates with the subsidence of the hot stage or exacerbation in the remittent, as proved by the general symptoms and physical signs, and follows, as it were, the phases of its associate; in other and probably the larger number of cases, the disease only abates to a greater or less extent, but does not cease in the first mentioned form, and does so but slightly in the latter, thus showing its independant existence. Finally, in such complications, it has not unfrequently happened, that the periodic fever has been arrested, while the local inflammation has continued to run its course, in the usual way, to recovery or death. * * * The fact of malarial fevers engrafting themselves on other complaints, and imparting to them the character of periodicity, is perfectly well known and generally admitted. It is one of such frequent occurrence as to surprise no experienced and observant physician, and illustrates while it confirms what has just been said relative to the complication of pneumonia with some or more of the phenomena of autumnal fevers." (*a*)

In the leading works on practical medicine that have been published in this country during the past quarter of a century, and that must be familiar to the present race of physicians, there may be found very clear descriptions of pneumonia when it is complicated by the evidences of a malarial or periodic element. In Eberle's treatise on the Practice of Medicine, we find under the caption of *Pneumonia Biliosa*, a very good account of the affection when the gastric and bilious symptoms predominate; and these symptoms are represented to be "*very frequent and prominent*" in that sketch which Dr. Manson has presented as the *type*

(*a*) Op. Cit., 437-8-9, 445, 451-2.

form of the disease. Indeed, he says, "the *evidences of hepatic and gastric disorders* have gained for the *malarial* form the *epithet* of *bilious.*'"

Eberle's description of *Bilious Pneumonia :*

"This variety of the disease occurs during cold and variable seasons, in districts abounding in sources of miasmatic exhalations. It appears to be the result of the combined agency of *koino-miasmata* and atmospheric vicissitudes. The initial symptoms of this modification of the disease differ very little from those which usually usher in an attack of ordinary bilious remittent fever. In some instances, a sense of fullness and tension is experienced in the right hypochondrium, a few days previous to the supervention of the disease, and occasionally dysenteric symptoms occur before the fever commences. In almost all cases, considerable pain is felt in the back and extremities during the premonitary period. The skin from the beginning is more or less tinged with bile, and the conjunctiva, especially is often conspicuously icterode. The face is flushed, 'and a sickly mixture of red and yellow, upon close examination, betrays the existence of a disturbed state of the liver' (Potter). Acute pain in the forehead is almost constantly present. The pain in the chest is sometimes extremely severe and pungent; but more commonly it is obtuse and attended with a sense of weight or oppression in the breast. In some cases, the fever continues for several days before the pectoral pain supervenes. The expectoration is not very copious, the sputa being of a frothy yellowish appearance, marked frequently with streaks of blood. The fever is generally attended with *manifest evening exacerbations and morning remissions;* when vomiting takes place, an *occurrence very common in this affection*, more or less of *bilious matter* is generally thrown up, although, in some instances, the secretion of bile appears to be entirely suspended ; the ejections consisting of nothing else than gastric mucus and the injesta. The tongue is at first white, with a yellowish streak along the middle, which, as the disease advances, becomes dark brown and dry. The urine is always of a deep yellow or bilious color, and the pulse is generally small, frequent, and quick, with a slight degree of preternatural tension." (*a*)

We have not unfrequently seen cases of pneumonia in this section of country, of which we regard the above as no meagre description. In the opinion of Dr. La Roche, it "embodies all that need be said on the subject."

The learned editor of Watson's classical Lectures on the Principles and Practice of Physic, Dr. Condie, has appended an able and highly instructive note on typhoid and bilious pneumonia, to the latter of which we refer the reader for a fuller delineation of the disease than that given in the above extract from Dr. Eberle. (*b*) No one can fail to perceive that both of these observers had in view the same form of disease which Dr. Manson has described under another title.

Dr. Wood has not failed to give a faithful account of this variety of pneumonia in that excellent work on the Practice of

(*a*) Op. Cit., Vol. I, 308-9.
(*b*) Op. Cit., 3rd. Amer. Ed., 605.

Medicine, which, as a whole, has no equal in our language. He portrays its distinctive features in his wonted clear and accurate style.

Wood's description of *Bilious Pneumonia :*

"By far the most frequent form of bilious pneumonia is that which occurs as an associate of remittent or intermittent miasmatic fevers. This is a very common disease in the misamatic regions of the United States ; and is especially prevalent in the cold seasons, in the low lying parts of the western and southern sections of our country. The pneumonia may either be secondary, making its appearance after the fever has set in, or it may be primary, and, by the disturbance which it excites in the system, call the pre-existing tendencies to the miasmatic fever into action. It occurs usually in the latter part of the autumn, in the winter, or in the early spring months, and appears to be caused by vicissitudes of the weather, acting on a predisposition acquired by exposure to malarious influences during the summer and early fall. It is most frequent and fatal in seasons which follow an unusual prevalence of bilious fever. It is very common among the blacks and colored population of the South. The name by which this complicated disease is perhaps most generally known in our country, is, *bilious pleurisy.* From the frequency of acute pain at the commencement, there can be no doubt that, as in ordinary pneumonia, the disease of the lung is often attended with inflammation of the pleura ; and it is possible that, in some instances, the inflammation may be confined chiefly to that membrane ; but generally the lungs are most deeply involved, as shown by the bloody expectoration, the physical signs during life, and the result of post-mortem examination.

The pectoral symptoms and physical signs do not materially differ, in this form of pneumonia, from those already described as characteristic of the uncomplicated disease. It is not necessary, therefore, to repeat them. The peculiarities are those derived from the miasmatic complication. In those cases in which the fever is the primary affection, the chill and febrile symptoms generally precede those proper to the pneumonia for one, two or three days, and sometimes a longer period. In other instances, the pneumonia and bilious symptoms pursue an even march. When the complicated disease is established, we have usually, besides the signs of pulmonary inflammation, more or less of the following symptoms: viz., headache, a yellowish, or yellowish white fur on the tongue, nausea and vomiting sometimes of bile and sometimes of the ordinary gastric fluids, a more or less yellowish tinge of the skin and conjunctiva, often a brownish color of the face arising from the mixture of the pneumonic redness with the icterode hue, and a deep colored, yellowish brown urine. The fever is always *remittent* generally with *daily paroxysms*, which, however, are often more severe on the alternate days ; and occasionally the remission is so considerable, that the disease appears almost in the form of a quotidian or tertian intermittent. In the latter cases, we may consider any existing fever in the intervals as sustained by the pulmonary inflammation. The grade of inflammatory action is in general less elevated than in ordinary pneumonia, and sometimes the fever assumes a typhoid character, the pulse becoming feeble, the tongue brown and dry, and the mind often somewhat wandering. The cases of intermittent pneumonia, spoken of by writers, probably belonged to this variety of the disease. Under proper treatment, bilious pneumonia generally terminates favorably ; though it may prove exceedingly fatal, when associated, as it sometimes is, with the more malignant forms of miasmatic fever." (*a*)

(*a*) Op. Cit., Vol. II, 17, 18.

It will not be denied that Dr. Wood is a "standard authority." His work has already reached the fifth edition within the space of eleven years, and may be almost considered the Prayer Book of the American Medical Practitioner. How succinct, and yet how comprehensive, is his description of bilious or malarial pneumonia? The pectoral symptoms and physical signs are so similar to those which he had already described as indicative of the ordinary uncomplicated form of pneumonia, as to render their repetition unnecessary. The grade of the febrile action is not generally asthenic, and it may be asthenic–typhoid, and is always of a remittent character, recurring usually with daily paroxysms. For the most part, there are marked tokens of biliary disturbance; and every leading peculiarity reflects the impress of the periodic fever element.

We have seen already, that the philosophic and lamented Drake has noticed this modification of pneumonia in that great work which he bequeathed as a dying legacy to the profession he so loved and adorned. It will not be expected that we should introduce here the entire section he devotes to its consideration; but we must make space for his portraiture of the prominent features of the disease.

Drake's description of *Bilious Pneumonitis*—

"In reaching a diagnosis of this malady, it is not necessary to travel through the symptomatology of either autumnal fever or pneumonic inflammation, but simply to refer to some modifications of both.

1. The signs of an intense phlogistic action are less conspicuous than in ordinary pneumonitis. The functional disturbance of the lungs may be quite as great as in the most acute inflammation, but it results largely from sanguineous engorgement, and retarded transmission of blood through the organ.

2. The fever, instead of the continued type of the pulmonary phlegmasia occuring in those who have not received a malarial or autumnal fever impress, is subject to remissions, even when most intense, while in numerous instances, it displays perfect quotidian and sometimes tertian intermissions. The pulmonary affection follows the same law, when it consists in well developed inflammation, that condition does not, it is true, cease with the periodical abatement or cessation of the fever, but is greatly moderated to undergo revival with the fever; but when the inflammation is less intense, and the pulmonary lesion consists chiefly in passive congestion, it may so far disappear in the more or less perfect apyrexia, that the term intermittent becomes almost as applicable to the local as the constitutional affection.

3. Such is the derangement of the biliary function in autumnal fever, whether the attack be original or occurring as a relapse, that in a large proportion of the cases now in our contemplation, bilious symptoms are decidedly present. In some the secretion of healthy bile is excessive; the fur on the tongue assumes a dirty yellowish tint, there is a bitter taste in the secretions of the mouth, vomiting and purging of bile occur, and now

and then eructations of that fluid. Its presence is also manifested in the urine, the serum of the drawn blood, the white of the eyes, the skin, and as I believe, but not on conclusive evidence, in the sputa from the bronchial tubes. In other cases the signs of a bilious deterioration of the blood are present, but there is no excretion of bile from the liver, which seems torpid, and by not pouring its bitter and alkaline fluid into the bowels permits the occurence of an exhausting diarrhœa, the discharges being watery, and so acrid as to excoriate the sphincter. * * * Some of the most dangerous cases are the least inflammatory and most intermittent, presenting, in their phenomena and mode of termination, a great resemblance to malignant intermittents. Those in which the fever most affects a continuous type and the pulmonary affection is the most decidedly inflammatory, are in general the least dangerous, because most amenable to treatment." (*a*)

The reader will be amply repaid by perusing the whole section from which we have made the above extract; but enough has been quoted to show, in the clearest manner, that the able author fully appreciated the malarial complications of pneumonia, and the modifications thus impressed upon it; and that his notice of the subject cannot be justly deemed either "cursory" or very "imperfect."

All the authorities from which we have now made quotations are readily accessible to those interested in medical affairs, and are doubtless in the possession of a large proportion of the physicians in this country; and, with all due respect, we must say, that, if the "neophyte" is not guided through the labyrinth before him by the lights thus hung out to illumine and make plain his pathway, we fear he must also be lost, though led by the hand of our gifted author.

Lastly, we must introduce the greatest of all "modern standard authorities" on the subject of pneumonia. In his great work, so often referred to in this paper, M. Grisolle has given under the caption, *pneumonie intermittente et remittente*, nine pages to the elucidation of the disease in question. He alludes to various authors both ancient and modern, who have written on the connection of pneumonia with periodic fever; and notices the affection as appearing under the two divisions just mentioned.

Grisolle's account of *Intermittent Pneumonia*—

1. "As is the case with most unpleasant complications, pneumonia is sometimes only developed after one or several paroxysms of simple, benign intermittent fever; but this is quite rare. In the greatest number of cases, there are to be seen from the first, some thoracic symptoms, and especially a pleuritic pain, which should fix the attention; but the disease is not entirely characterized until during the succeeding exacerbations. At other times the pneumonia commences as though it were simple, and it is only as it progresses that the specific cause, which produced it, or complicates it, can be recognized.

(*a*) Op. Cit., 869-70-71.

"The disease commences with a chill, which is more or less violent, during which the patient complains of pain of the side; soon there is cough and bloody expectoration, and the examination of the chest reveals fine crepitation; finally after a variable length of time, the fever declines, the thoracic signs diminish in the same proportion, and last of all there is abundant diaphoresis. The intermission may be complete, that is to say, the fever, together with the greater part of the local symptoms ceases; this is generally the case after the first or second paroxysm. At other times, during the intermission, some symptoms such as slight cough, a feeling of oppression or dull pain, a small amount of crepitation or dullness, continues to exist. In general the fever is of quotidian or tertian type; in the majority of cases, after a remission of twelve or twenty four hours, a new paroxysm, longer and more severe than the first, appears, and as the paroxysms increase in frequency, so do the intermissions become shorter and less complete.

"The local symptoms of pneumonia may disappear during the intermission, and reappear with the succeeding paroxysm; but in these cases the disease had hardly passed the first stage; crepitation was the only sign furnished by auscultation; but it is almost certain that if the paroxysms come on for several days in succession, the pneumonia passing from the first to the second stage, would make continued progress, and only manifest an exacerbation at the hours at which the febrile intermittent paroxysms had previously appeared. It is a fact, moreover, rare and purely exceptional, that pneumonia even at the first stage, can completely disappear at the termination of the paroxysm; that cessation of inflammation which is observed in some membranous tissues, seems far more difficult in the case of parenchymatous organs and especially of the lungs. Thus, then, when the first stage is well established, when the crepitant rale exists, the pulmonary lesion cannot be resolved in the few instants during which the apyrexia lasts; but it persists, it even makes progress, and becomes of itself the exciting cause of a continuous febrile movement.

"In consulting the reports of periodical pneumonias contained in the annals of science, we will be convinced that the febrile exacerbations are almost never separated by a complete intermission; there exists on the contrary a continued febrile movement in part symptomatic of the visceral inflammation, which augments at the time when the periodic exacerbations are renewed; pneumonia is then said to be *remittent*.

2. "*Remittent* pneumonia is rather a rare affection, and is hardly ever seen except in marshy districts of country and under the influence of causes which ordinarily produce ague and fever; it can then be observed as an epidemic. In general, the disease commences with the ordinary symptoms of open frank pneumonia. The patients have a violent chill, soon followed by burning heat, cephalalgia, and a pain more or less severe in one of the sides of the chest. These symptoms are accompanied at the same time by oppression, dyspnœa, and cough; the expectorated matter exhibits often during the first hours a yellowish or rusty tint, and the examination of the chest by auscultation and percussion, permits us to discover and *circumscribe* the pneumonia. The febrile movement once established, continues without interruption, but it undergoes from time to time, at variable but regular intervals, *exacerbations* and *remissions*.

"The exacerbations are generally announced by slight shiverings, which are generally partial, and which are altogether absent in a great many cases. Soon, the general and local symptoms increase in intensity; the pulse becomes more frequent, the heat of skin increases, the countenance becomes injected, respiration is accelerated and more painful; anxiety, dyspnœa, and pain of the side augment; there is at the same time a great deal of agitation, and it is not rare to see delirium now supervene. The chest has rarely been explored during the existence of these phenomena; but it is evident that, at this period, very active disease is going on in the lungs. M. Grisolière, for example, has assured himself that, in these cases, a considerable portion of the lung has become completely hepatized in the course of ten or twelve hours. The exacerbations can be of a much graver character, and then be accompanied by a series of symptoms which give to the disease a

pernicious form. In these cases, the countenance of the patient is profoundly altered, indicating stupor, there is great prostration; the pulse is filiform, compressible and sometimes irregular, and is very frequent, or on the contrary it may be slower than usual. After having persisted during several hours with great intensity, these grave symptoms decline progressingly or all at once. At the same time, the pain of the side and oppression in breathing diminish, the pulse loses its frequency, the powers of the system increase, and we have diaphoresis which is more or less profuse. If, during the remission, the local and general symptoms are seen to diminish in intensity, it is quite rare that *one* or *two* of them ceases entirely. Besides, auscultation and percussion have proved that the pulmonary engorgement does not undergo any change during the period of remission.

Soon a new exacerbation declares itself, and the symptoms become more and more grave in proportion to the frequency of their recurrence. But, one circumstance in these cases merits attention; it is that in exploring the chest attentively, we do not see the pulmonary change making proportional progress. M. Grifoulière observed that the hepatization, which he discovered within the first ten or twelve hours of the disease, remained afterwards stationary, and did not invade a greater extent of lung after three or four paroxysms. The periodical exacerbations are sometimes established from the very commencement of the inflammation; at other times the latter seems to exist without any perceptible exacerbation for several days, and it is not until towards the fourth day, sometimes later, as the sixth, seventh, or eight, that regular exacerbations begin to be remarked, as if a new element had come to join itself to the primitive affection. The *time* at which the exacerbations come on, varies, but is regular; in the majority of cases they are quotidian; as was noticed particularly in the epidemics of Mayence and in the canton of Aubin; they are much more rarely double quotidian, or tertian, and I believe they have never appeared in the form of a quartan. The hours at which the exacerbations occur are not constant; J. P. Frank observed them generally during the course of the day, the remission taking place in the evening or night; whilst in the epidemic in Aubin, M. Grifoulière remarked that the exacerbations occurred once in the 24 hours, and almost always during the evening or night.

If the disease is to terminate fatally, the intermissions are less and less evident, and the phenomena which mark the exacerbations are more and more dangerous; finally, death ensues. In the epidemic in Aubin, if left to itself, the disease terminated fatally from the third to the sixth exacerbations, in the worst cases; and in the others a little later, or from the eighth to the tenth day. Death may take place by the extension of the pneumonia; at other times the patient dies during an attack of delirium, chill, or coma. If, on the contrary, the disease terminates in recovery, the exacerbations diminish in intensity and frequency, at the same time that the local symptoms improve." (*a*)

Such is the career of pneumonia when complicated by the impression of a malarial law, as described by Grisolle, condensed and arranged from the article referred to in his great work. How beautifully clear and simple is his delineation of the leading phenomena that signalize the action and dominant influence of malarial poison—how faithful is the portraiture of the most prominent and distinctive features, which the accidental infusion of this special element impresses upon the march of pneumonia? Let the reader compare it with the sketch which *Dr. Manson* has presented of the symptoms that mark the general course and pro-

(*a*) Op. Cit., 427-434.

gress of this complicated disease. Will the *French* author suffer by the comparison? Is *his* description of the affection "*sadly deficient for us*"?

About one thing, at least, there seems to be no room for doubt; authorities enough have been adduced to show that modern writers have not so entirely neglected the consideration of the subject in hand as Dr. Manson has affirmed. It is true, they do not view the complication of pneumonia with the diathesis of periodical fever in precisely the same light with Dr. Manson, and do not afford support to many of his peculiar doctrines; and it is yet to be demonstrated, we are persuaded, that in this they have erred; but it is proved, as we submit, that *this* "path" has been explored before; that, indeed, it is an old and beaten track; consequently—for that reason, and not because, as Dr. Manson alleges, it remains yet "*unexplored*"—it is still more "*open*" to all subsequent travellers. And if medical men here in the South have not made themselves acquainted with the "peculiarities" of this modification of pneumonia, and have been guilty of the "consummate and fatal folly" of "blindly following a treatment intended for varieties of disease they never witnessed," as Dr. Manson so emphatically declares, but of which assertion he has not furnished a particle of proof, then we alone are culpable, and have no one to blame save ourselves; for we must candidly confess, however mortifying it may be, that we have remained ignorant when knowledge was within reach—amid universal sun-light, we have groped and stumbled as if in the dark.

(*To be Continued.*)

SUPERFŒTATION.

Mr. Editor:—Quite an abnormal case occurred in obstetrics in this neighborhood a short time since, which may present some physiological points of interest to your readers. A negro woman, belonging to a gentleman residing near this place, aged 19, primiara, aborted—the fœtus being about six months intra-uterine life. The fœtus was dead, but of good size and well formed. The mother soon recovered and pursued her usual avocations.

But *six weeks* after her abortion, she was again seized with labor pains, and brought forth another fœtus with a placenta attached. This fœtus was about six inches in length, with head and extremities properly formed. It was supposed to be not less than three months old, and it could not have been more than four. One side of the fœtus, particularly the head, appeared flattened, as though it had met with resistance from some cause during its development. The case did not come under my observation, but I will vouch for the facts as above stated; and now let me ask do they not prove conclusively the possibility of superfœtation? It may be said, that it was a case of twins, and that the second one died at the third month while the first lived to the sixth. But if so, why were not both expelled at or about the same time? Is it not possible, that there might have been a *double uterus*, and that the second fœtus was alive when the first was expelled? In support of this view allow me to make some selections, which perhaps all of your readers have not seen; but here I will remark, that I was unable to ascertain whether the woman secreted milk or whether she had the usual flow of lochia after the expulsion of the first fœtus. "Dr. Maton records a case in the *Transactions of the College of Physicians of London*, (vol. 4, p. 161) of a lady who was delivered on the 12th of November, 1807, of a strong and healthy boy, and on the 2nd of February, 1808 (not quite three calendar months) gave birth to another boy at full time. Dr. Granville thought that the first child in the case of Dr. Maton's was prematurely expelled, and the other carried to the full period; but this is successfully refuted by Dr. Maton himself, and by Fonblanque in his *Medical Jurisprudence* (vol. 1, p. 264)." "M. Velpeau, in his *Elements of Midwifery*, relates the case of a Madame Biguad who on the 30th of April, 1748, produced a living male child, and on the 17th of September following, another infant, which was also alive and healthy. He also speaks of a woman named Artes, who, in 1796 gave birth to a child at the full term, and in five months after, to another at its full term." "Prof. Lobstein, in the *London Medical and Physical Journal*, tells us that he actually delivered a woman of two infants *one a month after the other;* and was able to convince himself that that she had *two uteri*, and to each *a distinct and separate vagina.*"

But here is proof conclusive:—"Dr. Morton delivered a woman of a *black fœtus of the eighth month, and of a white one of the fourth month.* Having attempted to extract the placenta of the first before the second was born, he discovered the passage was so narrow that it would not admit his hands; so that this woman must have posessed a *double uterus*, and to each a vagina." *New Orleans Med. and Surg. Journal, May*, 1858.

There are in several Eastern Museums, preparations of double uteri, in some of which both chambers were impregnated, while in others, one chamber remained intact.

Yours with much esteem,

W. C. SANKFORD.

Franklinton, N. C., Feb. 3rd. 1859.

COMPOUND FRACTURE OF THE MALAR BONE.

By D. S. Waddell, M.D., Pittsboro, N. C.

In the month of December, 1858, a slave aged some twenty-two years, was struck with a jagged stone directly upon the left malar bone, and was knocked down. The appearance of that side of his face was greatly changed. The eye was very prominent, the bone completely flattened; œdema of the lids and ecchymosis; and an incision extending from near the outer angle of the eye, to near one-third of the zygomatic arch, reaching the bone. He complained of severe headache and obstructed motion of the lower jaw; whilst blood flowed freely through the nostril and in the mouth.

After a careful examination with probes, etc., it appeared that a portion of the bone was driven in, and a part of the floor of the orbit so much raised, that at no point could an elevator be inserted. Dr. J. A. Hanks and myself then proceeded to reduce the fracture. Strong pressure with the fingers was made on the inner lower margin of the orbit (at the outer angle) and at the same time steadying the zygomatic arch. Dr. H. then inserted his finger into the mouth, passed it up to the lower portion of the malar and pressed it upwards and inwards—while the orbital portion was pressed downwards and outwards. This was fol-

lowed by a very perceptible change, the appearance of the bone being very nearly natural. Patient expressed himself relieved of the pain and "stiffness" in the joint. Adhesive strips and a pledget of lint saturated with cold water completed the dressing. The eye recovered its proper motion, whilst the sight remained unimpaired. Up to the present time the patient has every appearance of a perfect recovery. Eight surgical works were consulted relative to this fracture, and but little information gained on the subject. In fact, Dr. Smith, of Philadelphia, says it has been doubted by some whether this fracture can take place, but adds *he* has seen two cases of this accident.

OBSERVATIONS ON THE VOICE AND COUGH.

Translated from the French by Edward Warren, M.D.

I. When the larynx of a healthy man is ausculted, the normal vocal sounds strike with considerable force upon the stethoscope, and against the ear of the person listening. Along the thrachea, this resonance is less intense, whilst upon the thorax, nothing can be heard except a confused and incongruous buzzing. This natural echo or resonance represents all the changes of the voice. It is augmented when the voice is stronger and more sonorous; it is more distinct in the neighborhood of the large bronchial tubes; and it becomes more perceptible in proportion to the size of the breast, and the thinness of its walls. As a general thing it is equal upon corresponding portions of the two sides, though not unfrequently, more distinctly marked at the summit of the right lung, which is owing to the greater diameter of its principal bronchus. In disease, the resonance is either simply exagerated, or so modified as to produce certain abnormal modifications of the voice known as *tremulous*, *cavernous*, and *amphoric*.

1. Increased resonance of the voice, or slight bronchophony. This is an exageration of the ordinary resonance only to one degree beyond that which is possessed by the voice in its natural state, and is not true bronchophony. This sign indicates lesions similiar in kind as bronchophony, though different in degree.

2. Bronchophony is a resonance of the voice much more distinctly marked than in health. It is distinguished by its intensity,

extent, fixedness, permanency and coincidence with the bronchial respiration. This sign is met with in enlargement of the bronchial tubes, in pleurisy and in pulmonary induration, particularly. As bronchial dilatations but seldom present themselves, bronchophony may be considered as especially characteristic of those changes which take place in the density of the lung. It indicates then either the existence of pneumonia or of tuberculosis; and is consequently a proper diagnostic sign of these two affections, though much more distinct in the former than in the latter. It exists also in some cases of pleurisy, indicating, when persistent, either pleuritic effusion in combination with pneumonia, or tuberculous induration.

3. Ægophony is a peculiar resonance, characterized by a sharp tremulous, and jerking modification of the voice. The patient speaks as if he held something between his teeth and lips which interferes with the formation and the egress of the note. It is ordinarily distinguishable upon one side only, and in the lower half of the sub-spinal fossa; and even when it extends beyond these limits, its intensity is always greatest within them. It does however alter its locality with the progress of the disease producing it, as can be readily determined by examining a number of cases. It generally exists coincidently with *feebleness* or *suppression* of the vesicular murmur at the base of the lungs. True ægophony indicates a pleuritic affection, of a very serious character. If perceived only to a limited extent on one side, and associated with arterial excitement, an ordinary pleurisy may be diagnosed; if upon both sides without the concurrence of fever, and in connexion with general dropsy, hydro-thorax is most certainly indicated; and if developed during the progress of pulmonary inflammation, with continual changes of position as one of its characters, it is proper to regard it as symptomatic of pluero-pneumonia.

4. Cavernous Voice or Pectoriloquy. When the vocal vibrations seem concentrated in a cavity, the sides of which convey the sounds with more or less distinctness to the ear, that peculiar physical sign known as cavernous voice, is developed. Usually it is limited to the upper portion of the chest, and is coincident with the existence of the cavernous rale. Pectoriloquy indi-

cates the existence of a bronchial dilatation, or an excavation in the substance of the lungs, resulting either from tubercles, gangrene, suppuration, or apoplexy. In nine cases out of ten, however, the cavity is of a tuberculous origin, as is generally admitted.

5. Amphoric Voice—This is characterized by a resonance which very nearly resembles that peculiar metallic or cavernous sound, which is produced by speaking over the mouth of a large pitcher, about three quarters empty. It is usually associated with amphoric respiration, and like it, announces a pneumo-thorax—though sometimes a large pulmonary excavation.

II. When the ear is applied to the breast of a healthy man, who is in the act of coughing, a dull and confused sound is heard which shakes the whole pectoral cavity. This phenomenon is made up of a movement and a sound; and is distinguished with increased facility according to its locality or its violence. When heard in the larynx, the trachea, and the roots of the bronchi, especially in narrow breasted persons, it has somewhat of a cavernous sound, and produces a sensation like that made by the rapid passage of air through a tube. In diseased conditions, the cough assumes characters to which the names of *tubular*, *cavernous*, and *amphoric*, have been applied, each of which is indicative of a distinct pathological condition.

1. Tubular Cough.—When the ear detects a noise like that made by a stream of air passing with much noise, force and rapidity through tubes with *solid walls*, the cough is said to be *tubular*. It manifests itself in the same abnormal conditions as bronchial respiration, and like it indicates pulmonary hepatization.

2. Cavernous Cough.—When the resonance is stronger and more hollow than in health, the cough is denominated cavernous. It makes a peculiar, unmistakable, and characteristic impression upon the ear, whilst the lesion with which it is invariably associated is pulmonary excavation.

3. Amphoric Cough—This is characterized by a metallic resonance very distinctly marked. When it occurs in connection with the amphoric voice and respiration, it indicates either the existence of a pneumothorax, or of a large pulmonary excavation.

Note by the Editor—The above translation has been made, not for the purpose of throwing any new light upon the subject of which it treats, but simply as a means of refreshing the recollection of those who, though materially interested in the matter, have given so little attention to physical exploration, that its most important principles have escaped their memory. We do not mean to disparage those who rely upon other sources of information in the diagnosis of disease—for we know there are some men of practical and penetrating minds who have attained a degree of proficiency in deciphering rational signs, that is not only wonderful in itself, but which can put the vaunted skill of many a self-satisfied auscultator to the blush. But we do affirm that the same acuteness and attention bestowed upon the science of physical examination, as instituted by Laennec, and enlarged and perfected by Andral, Roger, Skoda, Piorry and others, would have added vastly to the skill, success and reputation of the ablest of the rational school, and removed many a difficulty which nothing else could overcome. Every physician should labor to perfect himself in this art, by studying attentively all cases of pulmonary disease which come under his observation, and by permitting no opportunity for a *post-mortem* to pass unimproved.

DISCHARGE OF FLUID FROM THE UTERUS DURING PREGNANCY.

By W. R. Capehart, M. D.

Edenton, N. C., June 28th, 1857.

The subject of these remarks was a negro woman, aged thirty in her seventh pregnancy and eighth month. At midnight she was awakened by severe pains in the uterine region, and an escape of fluid at each paroxysm. Supposing herself in labor, I was sent for. Pains regular at long intervals, and an escape of fluid varying from $\frac{1}{4}$ to $\overline{3}$j. Notwithstanding these indications I found that labor had not commenced, and that fœtal circulation was distinct. Ordered a mild laxative and perfect quiet.

June 29th—Pains continued, at longer intervals; but were not strictly periodical, with a small escape of fluid during the morning. Fœtus felt distinctly, and fœtal circulation perfectly audable. In the evening pains ceased; as yet, no indication of any detachment of the placenta.

June 30th—She was so imprudent as to get up and walk to the house—150 yards. I saw her at 8 A. M. and sent her home. In an hour pains came on, (showing the importance of perfect quiet) with a repetition of all the above symptoms; slight escape at each pain. Fœtal circulation detected, and at times during the day unusual movement felt. With a view of auscultating the situation of the solution of the membranes, I caused her to occupy many positions, but they had no effect on the amount of fluid expelled—that depending entirely on the duration and severity of the pain. Treatment has been simply to attend to the sympathetic system, taking care not to administer any purgative that would excite a reflex action, either on the rectum or mammary glands, (oleum ricini will affect the mammary glands, which irritation would be reflected to the uterus,) light diet and never permitting the bowels or bladder to become distended.

Sep. 1st—Spent a good night; a few pains referred to the lower part of the organ, and a very slight flow of liquor amnii.

2nd—Spent a quiet night; felt but two pains and had no escape. Had some headache and fever; administered a mild laxative; Flo. Sulphur and molasses acted, and she felt no farther trouble.

I left for New York and was absent for five weeks. She felt no pains for two weeks after I left, when they returned and continued several days, with an escape of fluid as before. The treatment was the same, (perfect quiet, etc.) They again disappeared, leaving her perfectly quiet and comfortable. On the night of the 10th of August I was sent for, and found her in the first stage of labor. When the membranes were ruptured, there was a very small escape of fluid, but in all other respects the labor was perfectly natural. The infant was small and very thin, but showed no part that had come in actual contact with the uterus. There seemed to be just sufficient fluid to admit of a thin covering over the entire surface. It grew rapidly and is now a fine healthy child. The amount of liquor amnii discharged during the entire time prior to her confinement was about two pints.

Cause—This accident was occasioned by an attempt to lift a barrel of ashes on the 28th of June. This case, in many respects, resembles the case read before the Cork Medical and Surgical Society, Dec. 9th, 1857, by Dr. Harvy; but differs in several respects.

In this case there was a small escape of fluid with the child, whilst in Dr. Harvey's there was none whatever; it being, to use his language, "a perfectly dry labor." This case was attended by me in the summer before my graduation, and puzzled me greatly. Was this liquor amnii? I think it was—but have only to give a simple report of the case without comment.

Note—Watery discharges from the uterus during pregnancy are not unfrequent, though much difference of opinion exists respecting the nature of the fluid expelled. Some have supposed that it comes from the cervical glands or from the vagina, but this is evidently contradicted by the manner of its discharge—for it usually escapes in gushes. Drs. Denman, Burns and Pentland agree that the amnion is the source of the flow—though Petel has reported a case in which there was no dimunition of the liquor amnii, when for several months previous to the delivery, the flow had been excessive.

BIBLIOGRAPHICAL NOTICES.

Nature and Art in the Cure of Disease—By Sir John Forbes, M.D., D.C.L., F.R.S.

It is an old maxim, that "a man should praise the bridge which carries him over," and he who neglects or refuses to do so, proves himself unworthy the security he has enjoyed. Common gratitude demands some return at least, when favors have been bestowed, and substantial benefits rendered. Justice, though requiring an equitable judgment in all things, delights in the remembrance of friendly offices, and kindly deeds. This work, however, neither illustrates the truth of the maxim, nor vindicates the claims of gratitude and justice. A man upon whom the proudest honors of the profession have been lavished, who for

years has been the recipient of its noblest favors, who has been courted and caressed by the ablest of his peers, and who owes to Medicine all that he has of position, of fame, and fortune—ignores all his obligations, repudiates the principles of his whole life, and assails the vocation of his choice, as deliberately as if such a contradiction were an honor instead of a disgrace. For, disguise this fact as he may, the work before us is nothing more or less than an attack on Medicine,—a systematic and studied attempt to bring the profession into disrepute—a labored and ingenious effort to sap the very foundations of that art, which every consideration of honor should teach him to regard as the noblest and loftiest of all. But this is not all, this "legacy" is left, not only to those in the profession, but to the "intelligent laity" out of it—to the "public at large." Not satisfied with laboring to convince professional men of their ignorance, and the impotence of their calling, he addresses himself to "the people," to "the patients," to those who are "equally interested in the present system of medication," and seeks to perfect his triumph by destroying their faith in "Drugs and Doctors." This is "the unkindest cut of all," for though error is never dangerous when truth can combat it freely, the great difficulty of obtaining a fair fight in this unusual field, renders this assault upon the profession the more unjust and ungenerous. Arguments addressed to physicians themselves, can be analyzed fully, and answered fairly. The contest is an open one, and no improper advantage can be taken by either party. It is a free and evenhanded fight between truth and error, in which neither can complain of injustice or interference. But, when the public is appealed to for the purpose of settling the disagreement of Doctors, who can decide? Ignorant of the terms employed, unfamiliar with the principles of medicine, incapable of distinguishing between assertions and facts, influenced more by specious and sophistical arguments than by legitimate ratiocination, and carried away both by that prejudice which ignorance invariably entertains against real knowledge, and that agrarian spirit which but too frequently affects the popular mind, the laity are utterly incapable of deciding the questions discussed; and in appealing to them as umpires, the greatest injury is necessarily done to

the cause of science. Those out of the profession, have nothing to do with our abstract principles, with the theories, the hypotheses, and the speculations in which we choose to indulge; their business is with the practical results of medication—the ends accomplished by physicians, the actual fruits of our labor. And whilst we would esteem it the very acme of folly to attempt to prove to our "patients" that medicine is true, because confident of unsatisfactory result, we would vindicate the claims of our art to the confidence of men, by pointing to the triumphs of surgery, the inestimable boon of vaccination, the great discovery of chloroform, the delightful alleviations of opium, the wonderful effects of quinine, and the multitudinous blessings it has secured for the race.

The physician who reads this book must either find an answer to its arguments or acknowledge himself an imposter and his art a humbug. He must either believe with Sir John that "nature" accomplishes the cure and not medicine, or that it is effected by "art." If convinced that the first proposition is true, he must resign his commission, or retain it under the conviction that he is practising a deception; that he is speculating in the credulity of his fellow men; that he is only a miserable charlatan. That this is a legitimate conclusion, no candid man who has read the work carefully can question for a moment. For what are the principles inculcated with so much eloquence, ability, and zeal throughout its fascinating pages? He affirms that a majority of diseases are "self-limited," running a necessary and inevitable course, uninfluenced by any system of medication, and terminating invariably according to their own inherent and immutable laws. He asserts that many affections are necessarily fatal, whether subjected to treatment or left to themselves. He inveighs against specifics as the creations of imagination only, or the mere creatures of dishonest empirics. He affirms that Homœopathy—that most arrant of humbugs—has really accomplished positive good, by preventing the administration of such drugs as are usually invoked by the Profession in the management of disease. He rejects the idea that for each malady which flesh is heir to, a kind Providence has provided some appropriate remedy, sufficient in itself to meet every possible indication, and hidden only

because undiscovered by the fallible creatures interested in its application. He teaches a do-nothing policy; a let-alone philosophy; a quiescent, complacent, and most negative system of medication called "rational medicine" (?), by which the unfortunate patient is left alone in his misfortunes, until a favorable opportunity presents itself for the administration of an inert drug, given apparently for no other purpose than to secure the credit of the cure, should Heaven vouchsafe one. And now we ask in the name of common honesty, if the man who entertains such opinions as these, can practice his profession without involving himself in a most criminal or stultifying contradiction at every step of his career? Can *he* continue to pursue a vocation, who believes that the principles necessarily invoked in the discharge of its duties are untrue, without compromising his character as an honorable man? Does not justice, fair dealing and consistency demand an immediate surrender of his diploma, with all the privileges, prerogatives and emoluments appertaining thereto, by that member of the profession who has assailed it, by the inculcation of such heterodox doctrines as these? There can be but one response to these questions, and it is unnecessary to proclaim what that answer is, to those who read these pages. And yet, in direct contravention of his own opinions; in utter disregard of the principles proclaimed in his work, and in the most open opposition to the skepticism of which he boasts, he still retains the lucrative post of "Physician to the Queen;" a position in which he is necessarily compelled to prescribe in a legitimate manner, and in accordance with the practice of his peers. Here is a paradox which can only be explained by supposing either that Sir John Forbes is a charlatan, professing opinions or repudiating them as self-interest may suggest, or that he does not believe what he has written. This is a practical answer to all the fine-spun theories and elaborate arguments of this extraordinary book. This is the "*experimentum crucis*" by which the sincerity of the author is tested and determined. This is the most effectual means of meeting and repelling an attack on medicine, which is all the more dangerous because of the false colors whereby the assailant would lull its friends into security, and conceal his real character. For surely no man would permit himself to be deceived by representations emenating from one

who had proved himself either an imposter or a hypocrite, however specious their character, or with whatever plausibility urged. And no more convincing argument can be urged against the work, than that eliminated from the manner in which it has been received by the profession at large; for the practical test of each man's faith in its teachings, must be identically the same as that just applied to the author himself, and, as no great reform in the medical world has been effected; as no revolutions have to be chronicled; as losses from the true faith are as unusual as ever; as the ranks of the profession have not been weakened by desertions, we are constrained to believe that the laboring mountain has only produced a "*ridiculus mus*," and that in despite of the arrogant claims of this extraordinary production, it is a most contemptible and unmitigated failure. We are forcibly reminded of the old fable of the ox and gnat in this connection, but are pleased to discover that the profession can raise its head though oppressed by so terrible an *incubus* (*?*).

THE SCIENCE AND ART OF SURGERY—Being a Treatise on Surgical Injuries, Diseases and Operations. By JOHN ERICHSEN, Professor of Surgery and of Clinical Surgery in University College, and Surgeon to University College Hospital. An improved, royal octavo volume. *Philadelphia*—Blanchard & Lea, 1859.

This is one of the most complete and accurate works on Surgery of the present day; and from the rapidity with which the science has advanced, and the multitudinous additions recently made to it, the preparation of such a book must have been a most laborious task. Professor Erichsen has not only diligently studied the whole subject, so as to inform himself accurately in regard to it, but has selected his materials with excellent judgment, and used them to the best advantage. Without occupying time and space in attempts at self laudation, or wasting them in protracted discussions and profitless speculations, he has really presented the most able, concise and complete Treatise on Surgery known to the profession. Adding extensively to his former edition, both in letter-press and illustrations, he has supplied the desideratum so long experienced, of a convenient and reliable

work on surgery in all of its varied and important departments. Messrs. Blanchard & Lea have displayed their usual skill in the getting up of this book, and still farther added to the debt of obligation due them for their zeal and energy in presenting the best books to the profession, done up in the most handsome style.

Medical and Physical Commentaries, By Martyn Paine, M.D., A.M. Institutes of Medicine, By Martyn Paine, A.M., M.D., L.L.D.

These are works of vast research, of the most extensive erudition, and of wonderful ability, reflecting the greatest credit on their author, his country, and the profession of which he is a member. They embrace, in fact, the whole arcana of medical science, containing full expositions of every department included in the professional curriculum, presenting learned and erudite treaties on all topics of interest to the physician, and offering so wide a field for contemplation and study, as to fill us with surprise that one man could have accomplished so much. There is an unpretending simplicity in his style too, which is very pleasant and attractive, especially in these days of bombastic inflation and pedantic superfluities. In fact, Dr. Paine's works are a success, and not even the most carping critic can deny the fact without proving himself too ignorant and malicious for his office. As regards the great subjects of "solidism," "humoralism," "vitalism," &c., which are so extensively discussed in these books, we have neither the time nor space to consider them at present, but can only say, that Dr. Paine sustains his views with wonderful plausibility, erudition, and ability. No physician should esteem his library complete until these admirable works have been added to it, not as a mere ornament or for the name of the thing, but to be studied carefully and continuously, as well as in that spirit of exultation which the pre-eminent success of a fellow countryman must engender in every patriotic bosom.

REPORTS OF PRESIDENT, DIRECTORS AND SUPERINTENDENT OF THE INSANE ASYLUM OF NORTH CAROLINA, FOR YEARS ENDING NOVEMBER 1ST, 1857 AND 1858. *Raleigh*—Holden & Wilson, 1858.

We are under many obligations to Dr. Fisher, the accomplished superintendent, for this interesting document, from which we learn that the institution under his control is now in successful operation. As a Carolinian, we feel proud of this asylum—for it is really an honor to the State, whilst it reflects the greatest credit upon those patriotic men who in defiance of popular prejudices proposed and perfected this noble enterprise. For the actual condition, prospects and operation of the institution we refer our readers to the Reports themselves.

A PRACTICAL TREATISE ON THE DISEASES OF CHILDREN. By D. FRANCIS CONDIE, M.D., etc. Fifth Edition. Revised and Enlarged. *Philadelphia*—Blanchard & Lea, 1858.

The most positive evidence of the value of this work, is furnished by the fact that a new edition has been demanded for every three years of its existence. It is replete with sound pathological views, as well as admirable suggestions in regard to the general management of young children, and the proper method of treating their diseases. We cordially recommend it as a "standard" authority in relation to the subjects of which it treats.

A TREATISE ON HUMAN PHYSIOLOGY, DESIGNED FOR THE USE OF STUDENTS AND PRACTITIONERS OF MEDICINE—By John C. Dalton, Jr., M.D., Professor of Microscopic Anatomy in the College of Physicians and Surgeons, New York. With two-hundred and fifty-four illustrations. *Philadelphia*—Blanchard & Lea, 1859.

A complete "text-book" of Physiology, has long been a most important desideratum; and we are pleased to find that Dr. Dalton has supplied this demand in so satisfactory a manner. The great field of physiology—enlarged as it is by the explo-

rations of modern investigators—has been traversed with judgment and discernment, and its most valuable contributions to medical science carefully selected for the pages of this interesting volume. His division of the multitudinous phenomena embraced in this department, into those relating to *nutrition*, the *nervous system* and *reproduction*, is both sensible and convenient; whilst his treatment of the whole subject indicates superior ability and the most attentive observation. Critically examining the discoveries of others, testing them by personal investigation and experiment, and applying the crucible of rigid analysis to every new fact and novel theory, he has contributed no little to the real advancement of physiology by the production of this excellent book. To the student it presents all the most important facts of this interesting science, in the most convenient and accessible form; whilst to the practitioner, absorbed in the active and engrossing duties of his calling, it is particularly valuable, as enabling him to keep pace with the progress of science, without wading through ponderous tomes, or engaging in the laborious task of winnowing the grain for himself.

PRACTICE.

VERATRUM VIRIDE.

By James I. Rooker, M. D., (late resident Physician to the Commercial Hospital, Cincinnati, Ohio), Castleton, Marion County, Indiana.

Fewer articles of the Materia Medica are, at present, more the subject of thought and discussion among medical men, particularly in the United States, than the Veratrum Viride. I have before me some fifty different essays, that have appeared from time to time, in our medical journals; and, after a careful perusal of them all, I must confess that they leave my mind in confusion. While Dr. Norwood claims for it all the virtues of the far-famed Elix. Vitæ of Paracelsus, or the no less notable metalic tractor of Perkins, claiming it to be alterative in a high degree, also an expectorant, diaphoretic, nervine, emetic, and an arterial sedative, at the same time disclaiming for it any narcotic

properties whatever—we hear no less eminent physicians disagreeing with him on almost every point: saying they have seen a baby go promptly to sleep, after nursing, because the mother had taken Veratrum, advocating its use in all sthenic, and pronouncing it dangerous in asthenic cases, and *vice versa*. From the earliest days of medicine up to the present time, it has always been the case, when a new remedy has been brought before the profession, to use it indiscriminately, without any reference to physiology or pathology, until after long experimenting and close observation, its true modus operandi is established.

I therefore, deem it the duty of any physician, whether his experience be great or small with any new remedy, to publish his observations to the medical world, for the advancement of our science. While Resident Physician to the Commercial Hospital, I had the opportunity of giving and seeing it given in almost all cases wherein it has been recommended, and in all stages of the same diseases; and from my own experience, and from the recorded experience of others, it is to be viewed as a very dangerous remedy in all asthenic cases. In order to more fully illustrate its deleterious effect in asthenic diseases, I propose to transcribe from my wardbook a few cases, from a great number, showing its effects:

The first is a case of typhoid fever. Catharine S., æt. 35 years, admitted May 10th, 1857. German. Was taken sick one week previous to her admittance to the Hospital, with pain in her head and back, loss of appetite, general debility, with slight diarrhœa. Prior to the present attack, she enjoyed excellent health. When admitted, she plainly showed the typhoid expression: tongue dry, and dark sordes on teeth, loss of appetite, pulse 114, tolerably good in volume, abdomen somewhat tympanitic, bowels moved from three to four times per day. She was immediately put on four drops of Norwood's tincture every four hours, fifteen (15) grs. pulv. Dover at night. On the next morning, I found her very delirious; pulse 90, very weak; in every way much more prostrate than on her admittance. The veratrum, was discontinued and the following ordered: one-half bottle of ale per day, beef-tea ad libitum, with small doses of spirits of mindereri—

the former was continued up to May 12th, when we found her pulse up to 114, and resolved to again try the veratrum. On the morning of 14th, I found her pulse down to 84, hardly perceptible at the wrist: from this time, she gradually grew worse; pulse becoming weaker and slower, up to the evening of the 15th, when she died.

The second case was one of typhoid pneumonia, Jane P., æt. 22. Irish. Of intemperate habits. Was admitted May 2nd, 1857. Had been sick two weeks previous to her admittance; was taken with cold from exposure, followed by pain in the superior portion of the left breast; slight cough; some febrile disturbance. When admitted, complained of slight pain in the left breast; bad cough, expectoration very slight; respiration 24, pulse 120, tolerable full; tongue dry and dark; skin dry, hot, and dusky in color; appetite poor; complains of general debility; somewhat delirious; bowels free.

Physical signs: by percussion, we could detect dullness over the inferior portion of the left breast; by auscultation, we could detect bronchophony.

In this case, we gave veratrum. No sooner was the patient brought under its effects, than her pulse became much weaker and slower; and we were obliged to discontinue it, in order to save our patient.

Remarks.—I look upon these cases as being very instructive,—the first in particular. As soon as her system was brought under its influence, she became much weaker, and we were obliged to discontinue it. She was then put upon supporting treatment: her pulse grew fuller and stronger. The veratrum was again ordered, but, no doubt, to the great disadvantage of our patient. Veratrum is, at present, given by some, in the first stage of typhoid fever, and they assert, with the happiest results. Knowing, as we do, that typhoid fever is one that is (comparatively speaking) self-limited, and one of depression, for my own part, I can not see what indication they expect to fulfill by so depressing a drug.

I remember hearing a very eminent physician say, that he used it in all cases of typhoid fever, and that he had, many times, cut

the disease short in the first stage. *My reader, you know that it is often very difficult to make out a correct diagnosis, in the first stage of some fevers.*

Others assert, that they give it in the first stage of fevers, in order to quiet the heart's action; and, by so doing, they prolong its strength. My reader, allow me to *reply* to this argument, by this simple comparison : Would you think, for one moment, of overloading your horse, when you start on a journey, in order to prolong his strength ? You answer, No! But veratrum is given every day, with no other intention than to paralyse the heart's action, that it may be better able to perform its duty in the last stage of disease.

You naturally ask, in what diseases would you use it ? I might use it in the first stage of some inflammatory disease, where I had not at hand more potent remedies.

It appears to me, it would be an excellent remedy in acute hemorrhage; and I have seen it used in some few cases of the kind, and with the happiest effect. It is used by some in pneumonia, no doubt, with some good; but who would think of substituting it for tartar-emetic ? In all, I have a very poor opinion of Norwood's Tinct. of Veratrum. I do not like the drug itself, or the looks of the bottle he puts it up in. It is my humble opinion, that Dr. Norwood will reap more benefit from it, than all the rest of the doctors in the United States. And I have no doubt, could it speak for itself, it would cry in tones of thunder, save me from my friends.—*Cincinnati Lancet and Observer.*

INFANTILE CONVULSIONS.

When a child has convulsions, says M. Trousseau, learn to act quietly, indulge in no tumultuous proceedings; inquire if your patient is subject to such accidents, and if they generally pass off of themselves; if so, then little medical interference is required, and, generally speaking, the convulsions preceding eruptive fevers will also quietly pass away. Bleeding, prolonged baths, violent purgatives, vesications made by boiling water, so far from being of service, aggravate the disease, interfere with its

progress, retard the eruption, and occasion the complications. Persons, strangers to the art of medicine, and even physicians themselves, often act in a very absurd manner. They pour boiling water over the legs of unfortunate children, and thus occasion more serious diseases than the evil they desire to avert. This boiling water, thus brutally applied, is it not the frequent cause of those horrible scalds, which cause the death of so many children? What physician has not seen them? Sometimes the doctor comes and envelops the legs of the wretched child in linen wrung out of boiling hot water. The child is comatose, and feels not, and thus the patient is killed instead of saved. I saw one, who was my master—Marjolin—thus treated; boiling water was applied to rouse him out of the stupor which had supervened in the course of a typhoid affection; he suffered in consequence from deep ulcerations.—*Dub. Med. Press.*

ON THE USE OF HYDROCHLORATE OF AMMONIA IN NEURALGIA.

By Horatio C. Brenchley, Esq., M. R. C. S., Surgeon to the Brighton Dispensary.

The hydrochlorate of ammonia, although a very old medicine, has been very rarely employed internally. I cannot call to mind having once seen it so used during the whole time I have been in the profession. It has, however, as you observe, been long in use in Germany, though chiefly as an alterative and resolvent; but, recently, it has been much in repute in India for the cure of neuralgia. I can bear testimony myself to its great value in this painful affection.

The method of exhibiting it is in doses of half a drachm, in camphor-mixture or spearmint-water, every hour during the paroxysm, until the pain is relieved; and it is affirmed, that the prolonged use of it, in smaller doses, for a few days, produces immunity from future attacks.

In order that I may not occupy too much of your time and space, I will relate but one case:—A young man, aged twenty-three, unmarried, healthy, and without any other apparent com-

plaint, had long suffered from very severe attacks of neuralgia of the face, coming on at intervals of about one month, and lasting from two or three days to a week. It sometimes came on on one side of the face, and at other times on the other side. During one of these attacks I saw him, and ordered him quinine and arsenic, which put an end to the attack for the time. After the lapse of a month or six weeks, he had a second attack, which was cured in the same way. A third attack, however, came on, after a shorter interval. This time the quinine and arsenic failed to relieve him. On the third day, when I saw him, he was in great agony, propped up in bed, and unable to do anything from the severity of the pain. The left side of his face was swollen flushed and hot—the temperature considerably higher than on the right side; the heat also of the inside of the mouth was so great, that I expected I should find matter forming from decayed teeth; but, on examination, I failed to do so.

I now tried the much-vaunted remedy—the valerianate of ammonia, but without any effect. The usual remedies having failed I gave him the hydrochlorate of ammonia, in doses of half a drachm, every hour, in camphor-mixture. I saw him three hours after he had commenced this treatment, and found he had been much relieved after taking the second dose; and, having taken the third dose, he was almost free from pain, and begged to be allowed to continue the remedy. The heat and flushing of the face had subsided, and the temperature of the mouth considerably reduced, feeling quite cool after the burning heat of its former state. He went on for three or four days with the remedy, in doses of fifteen grains, three times a day, although there was no return of pain. Three months have now elapsed, and he has had no relapse.

The *modus operandi* of this medicine is not very clear; but whatever other specific virtues it possesses for the cure of neuralgia, in this particular case it evidently acted as an indirect sedative, by lessening the arterial action; for the first and most striking effect of the medicine was the rapid lowering of the temperature of the mouth and face. From further observation I have found that this remedy is most useful in those cases of neuralgia which are attended with heat and swelling.

From what I have seen of its action, I shall, for the future, always feel that we have, in the hydrochlorate of ammonia, an addition to our list of remedies for neuralgia which is most useful, safe and efficient.—*London Lancet.*

POISON OAK (Rhus Toxicodendron.)

By F. P. Wierzlicki, M.D., of California.

THERE are several species of the "poison oak" known to naturalists. I have met with but one species in California. *Rhus toxicodendron* infests the whole country, from San Diego northward. Where nothing else would grow, it flourishes, and flourishes almost as well as it does in rich soil, where it frequently attains the height of ten feet and the dimensions of a large bush. We can safely say that a person cannot walk in the fields without treading upon, or brushing against its ever-green branches at every step; so widely spread is the noxious shrub. Some writers make a distinct species of what they call *Rhus radicans.* I agree with Dr. Bigelow that there is no such species; it is the same *Rhus toxicodendron* that becomes *radicans* when there is a support close at hand, around which it can entwine itself; otherwise its stem grows sufficiently stout to be erect, and face the winds of the heavens.

"There is, however, another species of the shrub in California, in the latitude of San Diego and southwards. My impression is, that this species has not yet been described. Certain it is, that it does not correspond in appearance, either with *Rhus vernix* or *Rhus pumilum,* which are both considered very poisonous. It grows in that region very luxuriosly; is evergreen and attractive in its looks, like its cousin of the north. It emits, however, an extremely offensive stench, as a warning, as it were, to the passer-by, not to approach, for it is the most virulent of its class in its influence upon the human system.

"The poison oak at night, or when it grows in a shade, emits, according to some, hydrocarburetic gas with some acrid vapor; and it is this which is supposed to be noxious. Whatever may be the nature of this exhalation, it is certain that it is not confined to night or shade alone, as it is equally pernicious even when it

grows in broad sunlight. These exhalations seem to float in the air and are absorbed into the system either through the lungs or the skin, or through both at the same time. They are the most noxious, the most virulent, when they come in the shape of fumes arising from the burning shrub.

"The poisonous effects of the *Rhus toxicodendron* are manifested first upon the skin by a slight eruption, preceded by itching. This eruption may be considered as erysipelatous inflammation. Though the eruption is always characterized in the incipient stage by the erysipelatous blush, it changes its form according to the part affected. Sometimes it presents itself in vesicles scattered without any regularity over the surface of a part affected. Sometimes it forms only an inflamed line, not unlike a scratch. Sometimes, as when on ankles or wrists, it appears like a bracelet encircling the part, and on feeling it, gives the idea of a small shot lodged under the skin. These vesicles maturate, if not interfered with early; they become inflamed, and the part increases in swelling till the skin breaks and matter issues. No part of the body is exempt from being affected by this eruption, and any slight circumstance determines its location. A scratch, a part in perspiration while exposed to the air, a sprain like that of the wrist, is sufficient to locate the affection, when a person is exposed to the exhalation of the shrub. Its favorite location is the face, arms and *pudenda*.

"The degree of susceptibility to the exhalations of the shrub are different in different persons. While there are some who are never effected by these exhalations, even by touching the shrub, others on the contrary, cannot without the risk of infection, show themselves in its proximity. There are some persons who at one period were not susceptible to the poisonous effects of the shrub, but who have susequently become very susceptible to them, and this susceptibility seems to increase with each new attack of the affection. I am of the opinion that in many cases the *virus*, by frequent exposure to it, has been accumulating its force in the system before it breaks out, and by the treatment usually pursued, it is not eradicated thoroughly; and hence those frequent attacks during a season favorable to it, or those periodical returns of the affection which some people experience. The success of my own plan of treatment confirms me in the above opinion.

"Merely riding through the field where the shrub grows, is enough to cause some persons to be attacked by its poisonous exhalations. Those who are very susceptible to this influence, can frequently, by a peculiar sensation they experience in the nostrils, tell whether they are going to suffer its effects. The excessive itching of the part affected soon removes the doubt, if any exists, in relation to the matter, and the itching, together with the blush already alluded to, are unmistakable symptoms of this affection. The itching accompanies it through its whole course.

"Handling in any way this poisonous shrub exposes the susceptible person to an attack of its *virus*. Persons touching cloths against which a branch of this shrub has rubbed, handling firewood to which a twig did or does adhere, are frequently equally liable to suffer. Walking in the field where the shrub has been destroyed, and only some fragments remain on the ground, may expose a person to an attack. Some pretend to say that contact with the shrub is necessary to produce poisoning; but they are entirely mistaken, as I can assure them from my own personal experience and observation.

"The degree of intensity of the affection varies according to the peculiarities of the constitution, from the most trivial to the most serious symptoms, sometimes involving the whole frame, sometimes endangering life, and even bringing on death. Several fatal cases have happened in this country among new comers; but the deaths in such cases were owing to the mismanagement of the practitioner. Death should be an extremely rare occurrence in this affection, if properly treated.

"There are various plans of treatment of this malady, both among physicians and the people. Some of them are innocuous, and if they sometimes succeed, it is because the malady gets cured of itself. Others again are mischievous, and may prolong the suffering, or forever disfigure the part affected. They may leave unseemly scars, or minute capillaries engorged with blood, which thus present a sort of network of red lines on the surface, which never can be destroyed by any subsequent applications.

"I do not, however, propose to detail here the means resorted to for the cure of this malady by others. I will confine myself to stating the principle upon which any remedy can succeed or

fail; and then I will indicate the method I rely on, in the treatment of this affection. The principle upon which any means may succeed in this malady, must have for its object either the prevention of the development of the vesicles in the very inception of them, as a moderate degree of cold steadily applied, or their destruction by some counteracting agent brought in contact with the surface affected. It is upon this latter method that I depend, so far as the destruction of the *virus* externally is concerned; but as this, on many occasions, is not enough, I have then recourse to some of the natural functions, such as the skin and the bowels. As in this affection capillary vessels are engorged, alkalies of moderate strength are the proper remedies for the cure of the affected surface; for they not only by their action upon the skin, serve as gentle counter-irritants, but being absorbed in a measure by the skin, they cause liquefaction of the engorged blood. Such being my views of this malady, I have no difficulty in combating it successfully.

"On the very first appearance of this affection, wet the part well, and sprinkle it plentifully with powdered commercial soda, known to housekeepers as 'washing soda,' and let it dry. One or two applications of this sort in the course of twenty-four hours, will cause the incipient eruption to vanish. I prefer this impure carbonate of soda because it is stronger in its alkaline action than the carbonate of soda of the apothecary. In any stage of this affection the same means can be used, provided regard is had to the strength of the alkali; for when the skin is broken, and the vesicles exude matter, the strong alkali causes unnecessary suffering to the patient. Washing, therefore, with a warm and moderate solution of the same will best subserve the purpose. Where running sores have been formed, after washing the part with the above-mentioned solution, dry lint should be applied to it, which is to be removed on the next morning's dressing, by means of soaking the part in warm water first.

"The above mode of procedure is the most simple, the most rational, and the most successful, of superficial means, for the cure of this troublesome affection. If the symptoms, however, should be of a grave nature, internal remedies should also be resorted to, and the following in preference to any other: *milk of sulphur* with *bitartrate of potassa*, in quantity to suit the case. Any other

saline cathartics will do well. Sometimes even an emetic may be absolutely necessary; the judgment of the practitioner should decide upon its propriety.

"As an external application, when the above-mentioned alkali cannot be procured, the most ready remedy may be found in warm water, in which a large quantity of table salt has been dissolved: it should be as warm as the patient can bear it. Washing the affected part with this solution for several minutes at a time, morning and evening, relieves the excessive itching, and though slowly, yet surely, cures the malady. Weak *aqua ammoniæ*, applied with a feather to the affected part, is also a good remedy, though not equal to the carbonate of soda. Holding the part affected to the heat of a live coal, as near and as long as can be endured, a few times, causes the eruption to disappear, if it be in the very incipient stage. This remedy is not, however, of an equal effect and adaptability to every part of the body. It should not be applied to the face, on account of exposing the eyes to the heat, and because that part cannot be conveniently kept covered up after the application. The view I hold of this affection, authorizes me to recommend soap liniment, to be applied with a feather to the part affected, as a remedy that promises success. I have not tried it yet, but would not hesitate so to do the first opportunity, since I am persuaded that my view of the malady is the correct one, and the means would meet the indications of the case.

"Before I leave this subject, I have yet to mention what I should call a *sovereign remedy* for such cases—I mean the vapor bath. One or two baths are sufficient to destroy the *virus* of this shrub in the system, by throwing it off through the pores of the skin. The vapor bath also acts as a preventive means against the exhalation of the shrub, since the system becomes less susceptible to their influence, for reasons already pointed out above. Persons that are very susceptible to this *virus*, should take a vapor bath once a month or so, if they wish to be free from this annoying affection.

"There are many prejudiced against vapor baths, because they are supposed to expose the patient to taking cold more readily. This is a mistake. Imprudent people are always running this

risk under any circumstances. Allow your body to cool gradually; do not go out into the open air as long as the face is kept moist with perspiration; change your linen as often as it becomes uncomfortably damp from the perspiration of the body; keep for a few hours a little more warmly clad than under ordinary circumstances, and then there will be no risk of taking cold in consequence of a vapor bath. There are persons whose conformation, or the condition of whose lungs, does not permit them to breath vapor without great inconvenience or even danger. Such subjects should take the vapor bath by means of those chairs which are constructed for the purpose, and which enable the head to be kept in the fresh air.

"It will be perceived that I do not follow any of the writers upon the subject of the malady caused by the exhalations of the *Rhus Toxicodendron.* I found their notions on the subject so absurd and unphilosophical, both in theory and practice, that I do not hesitate to discard them, considering that my experience and observation upon my own person and the person of others, will be a sufficient apology."

THERAPEUTICAL PRINCIPLES.

Dr. Henry Kennedy thus concludes an able article in reply to Professor Bennett, of Edinburgh, on the Change of Type Theory :—

1. That both animal and vegetable life is subject at times to epidemic influences, which at one period raise, and another depress, the standard of health.

2. That the older writers, aware of these dangers, altered their treatment accordingly.

3. That pneumonia, like fever, alters its type at certain times; being now sthenic, and again asthenic; and that both affections are liable to spontaneous hæmorrhages, which are usually most salutary.

4. That no six cases of the same disease are identical, in either their progress or their termination.

5. That the very same inflammations may, and do produce different products, at different times.

6. That nature's cure is effected, if at all, by lowering the system; in other words, by antiphlogistic measures.

7. That in most cases where lymph and pus are poured out, nature does not, by herself, effect a cure: as exemplified in cases either of pneumonia or pleuritic effusion.

8. That statistics cannot guide us in the treatment of disease.

9. That every case must be treated on its own merits.

10. That by means of general bleeding, besides other effects, absorption is powerfully influenced.

11. That local bleeding is a powerful aid in curing disease.

12. That mercury is a most valuable agent in the treatment of some inflammations, and will succeed where other means fail.

13. That certain inflammations, including particular kinds of pneumonia, are very happily treated by wine alone.

14. That local bleeding may be used with very good effects, at a time when wine is also given.

15. That no single plan can possibly meet the ever-varying shades of disease—pneumonia amongst the rest.

16. That it is essential, in the treatment of all acute affections, to pay attention to the type of the disease for the time being.—*Edinburgh Med. Jour.*

RHEUMATISM.

By Skoda.

Formerly, it was the practice to treat acute rheumatism with warm applications from the dread of driving the disease inwards: now, we know that the affection of the internal organs is so far from being in any way opposed to that of the joints, that, on the contrary, the lungs, pleura, peri, and endocardium suffer most when the joints are most severely affected, though to this there are exceptions. From this it follows that no ease-producing applications are to be shunned from any vague dread of metastisis. Should warm applications give the desired relief, there can be no possible objection to them, but if not, as often happens, then we may at once with propriety resort to cold ones; and Skoda is satisfied that by doing so lung and heart disease are not increased, but greatly diminished, particularly if, under their use,

the pulse diminishes in frequency and the patient's restlessness decreases, as these are all the alleviations attainable under any therapeutics in rheumatism. As to other remedies, Sk. has but little to say in praise of them. When the joints are much swollen, leeches are not contra-indicated, but the relief obtained is not lasting; blood-letting (general) may also be occasionally usefully employed, but its repetition is injurious. Of internal remedies, Sk. has employed Sod. Bicarb., Iodine, Sublim. Con., Quinine, Tart. Emet., Digitalis, Pot. Nit., Ammon., Mur., etc., without much benefit. Quinine is only useful when the attacks are paroxysmal. Digitalis and Tart. Emet. are, however, occasionally useful in individual cases, though no distinct reason for it can be given, nor their success predicted. Narcotics are most useful in lessening the pain; and as this usually increases at night they are best given at bedtime; unfortunately, many bear these ill, small doses being followed by sickness, vomiting, and increase of pain.

(Dr. Kraft adds, that for many years ice applications have been successfully used in his department of the hospital at Prague, in the treatment of acute rheumatism, particularly when great pain is accompanied by greatly increased temperature of the parts.)—*Allg. Wien. Ztng.*

SURGERY.

LITHOTOMY.

By Valentine Mott, M. D.

THE common operation for lithotomy which is most in fashion, is the lateral, known from the time of old Frère Jacques. This operation divides the side of the prostate only; it goes parallel with it, and not above it, and does not open in the cavity of the pelvis. A man who would perform the operation with this reference, will be most successful.

I say, and I have said for a great many years, that all operations for the purpose of opening the neck of the bladder and cutting the prostate a little below this ilio-vesical fascia, are better done with some sort of a knife than with any other instrument

whatever. I have no wish to be arrogant in this matter. I have performed a great many more operations for stone than will be performed for many years. The number amounts to over 160, and I have never used in the operation anything but a knife and bistoury. On the whole, I prefer a straight bistoury. I was occasionally in the habit of using Mr. Thomas Blizard's knife. Of the gorgets that were introduced by Cline and Physic, I consider the one introduced by Physic as the best. Cline's is a dangerous instrument. I have seen him use it. Physic's is a dangerous one unless a man understands the relative anatomy; this was not in fashion in his day.

The knife goes through one side of the prostate; it passes below the loose layer of the fascia iliaca. I will also add, and this is contrary to the opinion entertained by many gentlemen, that I cut through the whole prostate. I never wish to tear it. If you are below this fascia you may cut to the ramus of the ishium without any danger.

Those who are not familiar with the use of the gorget, depress the hands, elevate the blades, and consequently it passes too high up, and occasions the danger alluded to. Dupuytren gave the subject great attention before he and his colleagues discovered the best plan. One took the recto-vesical, and the other the high operation.* All instruments that do not pass below this fascia are dangerous. I never cut the internal pudic artery with a knife, because I cut downwards and outwards in such a manner as not to interfere with it.

I am of the opinion that the operation for stone is most easily performed in the manner alluded to. Some cut through eleven-twelfths of the gland and tear the rest; but remember that lacerated wounds are hard to heal, and you had better make a clean cut. The idea has been entertained that by cutting entirely through the gland you cut this fascia, but having an eye to the course of it, there is no danger.

The bi-lateral operation is entirely free from the danger of wounding the artery; but I must say that I never saw the elder Cline, the preceptor of Sir A. Cooper, operate for stone that he did not cut that artery. I have seen the hemorrhage so profuse

* Dupuytren took the bi-lateral.

that the blood would run off the table in a stream and saturate the bed. I choose to avoid such consequences. These points about the prostate gland are eminently useful to us, and are of the utmost importance.

In regard to opening the bladder in different ways for evacuating urine, there is no danger except in the perineal cut. The operation *per rectum* is one unattended with danger, not involving the fascia; it is therefore to be resorted to when the bladder is to be emptied. One thing I will say, that during a long life —the many years that I have practiced surgery—it is gratifying for me in the retrospect, to recall this fact,—*that I have never punctured a human bladder*, though I have seen difficulties presenting themselves of the most serious character. I am always horrified when I hear that a surgeon has punctured the bladder. I have never seen a case of stricture that could not be relieved by the judicious application of instruments. In that disease confined to old people, I have never failed in being able to introduce a catheter, and where others have repeatedly failed, I have resorted to the long Sabatier's catheter, and have never failed in getting into the bladder.—*N. Y. Jour. of Medicine.*

NEW MODE OF DRESSING FRACTURED CLEVICLE.

In the *Chicago Medical Journal*, Dr. J. W. Freer gives his own method for the treatment of this fracture, which seems to be simple, easy of application, and to answer all the indications. He applies a strip of adhesive plaster, two and a half to three inches wide, and long enough to extend from the under surface of the forearm, near the elbow of the injured side, to the shoulder opposite, the strap being applied about its middle to the form, and passing each end, one in front and the other behind, crossing on the shoulder and lapping over. This must be drawn tight enough to bring the elbow firmly to the side and elevate the shoulders, a pad having been placed in the axilla to carry the shoulder outwards. Next, a similar strip is passed around the injured arm at the axilla, carried across the back under the arm opposite, and lapping upon the breast tight enough to bring the shoulder sufficiently far back. Finally the hand is supported by

a handkerchief, attached to a loop of plaster over the sound shoulder. If it be necessary to make compression over the seat of fracture, this may be done by means of a strap placed under the elbow and over the shoulder of the wounded side.—*Med. and Surg. Reporter.*

DISPLACEMENT OF THE COCCYX SIDEWAYS.

By Roeser.

A CORPULENT woman, aged thirty-six, fell from a table on a chair, so that its back came right between her thighs. She instantly felt severe pain in the coccyx, but continued able to move about till evening, sitting increasing the pain very much. In the evening the pain was so great, extending up the spine, that she was obliged to go to bed, and soon after could neither turn nor rise up. After a painful night, R. found this otherwise blooming woman quite immovable, with distorted features; she complained of violent pain in the coccyx, and a painful tensive drawing feeling from below, up to the neck, which also extended down the arm. She could move the forearm a little. The slightest motion of the body or head to one side was impossible, and still more so sitting up in bed; confused headache, and some mental disturbance, were also present. She made no complaints of her lower extremities, nor of her arms, and urinated without difficulty. After placing her on her right side, a small swelling, the size of a hazel-nut, was felt near the notch of the buttocks next the left ischium, which, on closer examination, proved to be the coccyx separated from the sacrum, and forced from the mesian line towards the left ascending ramus of the ischium. The obtuse end of the sacrum could easily be felt between the buttocks. By placing one finger in the rectum the dislocation of the coccyx could be still more easily felt; forcible pressure downwards and towards the right buttock caused it suddenly to glide into its normal position, whereupon the patient declared herself relieved, and as if roused from a dream, and all her pains vanished. She could move about freely; but pain in the sacro-coccygeal region prevented her sitting up; her expression was also com-

pletely changed. After a few days, a dull pain in the sacro-coccygeal region preventing sitting, was all the uneasiness that remained; and in five days, all of this that was left was a slight burning sensation at the injured spot. The irritation of the spinal marrow observed in this case, in which only the very lowest filaments could have been disturbed, and which, nevertheless, sent the stream of disturbance to the brain itself, is a most interesting example of mechanical irritation, as evidenced by its instant disappearance on the reduction of the dislocation.—*Betz. Memmorab. aus der Praxis.*

TUBAGE OF GLOTTIS.

THE following extract is from a report recently made to the French Academy—

"The *tubage* has then been practiced seven times: five times death has followed; the only two case which have been cured required tracheotomy, practiced *in extremis*. Certainly, such results are not encouraging, and M. Bouchut, who accuses tracheotomy so willingly, will have no right to think those severe who cast blame on the operation of tubing the glottis. We must, nevertheless, say with impartiality, that we do not believe that the *tubage* of the glottis was the cause of death in the seven children, no more than we will blame tracheotomy with the death of those on whom it is practiced. The *tubage* of the larynx has not killed one child; it appears in several to have retarded death, and we are certain that it has not accelerated it in any one. In our opinion, it would have been better to have practiced tracheotomy at the moment when all medical resources seemed worn out. Must we say that *tubage* is a precedure which we must reject altogether, absolutely? We do not think so, the operation is still new, and we must hope that each day will bring its complete success, and that, in a short future, the *tubage* of the larynx will be registered as a positive success. And if the future proves that in simple, acute laryngitis, which kills sometimes by laryngeal occlusion, *the tubage*, practiced during some hours, prevents death, and allows us some time to act with useful medicaments, M. Bouchut will have rendered to medicine a great service. How-

ever, your committee must limit itself to the following conclusions: 1. The *tubage* of the larynx, in certain cases of acute laryngitis, may, in retarding asphyxia become a curative means. 2. In certain chronic diseases of the larynx it may enable us to postpone tracheotomy, and sometimes to treat and cure the disease. 3. In the treatment of croup it postpones sometimes asphyxia, and allows us to introduce more easily into the air passages, agents capable of modifying the diptheritic inflammation. 4. It can but rarely supply the place of tracheotomy, which continues the chief means of treating croup from the time that medical means seem exhausted."

CALCULI IN THE HYPERTROPHIED GLANDS OF THE PALATE.

The *Union Medical* relates two cases of young persons sixteen and twenty-five years of age, who had for some time suffered from difficult breathing, nasal voice, difficult deglutition, and choking fits during sleep. On the velum palati, two or three swellings from the size of a pea to that of a nut, were visible; a fine sound showed these to be formed by hard stony bodies, on the presumption that these were formed of carbonate of lime, and consequently easily changed by sulphuric acid into friable gypsum, a mixture composed of equal parts of ac. sulph. and water, was partly injected into the small opening of the swelling partly pencilled over them. The result was gratifying; in a short time a grey friable mass escaped from the glands; the swellings were thus completely emptied and the remaining cysts speedily regained their normal volume under the use of alum gargles.

PAINLESS DENTAL SURGERY.

Dr. J. Smith concludes an interesting article in the Edinburgh Journal, thus:

From the foregoing observations, then, it would appear:—

1*st*, That the application of narcotic and other substances to the unbroken surface, or even their injection into the tissues, has as yet failed to render tooth extraction painless.

2*d*, That freezing, from the want of uniformity and uncertainty in its action, and the difficulty of its application, is also inadequate for this purpose.

3*rd*, That electricity, applied in any way yet practiced in dentistry, is not an anæsthetic; and that any apparent effects of this nature are due to causes quite apart from any such quality in that agent itself.

PHYSIOLOGY.

THE PHENOMENA OBSERVED IN DEATH BY HANGING.

By S. H. Tewksbury, M.D., Portland, Maine.

In compliance with the request of several of my professional *confreres*, I offer the following observations regarding the recent execution at Auburn. The accounts of the previous condition, and the general circumstances accompanying this tragic occurrence as detailed in the newspapers, have been so full, that I consider it unnecessary to add any thing in this connection.

As is already palpable to most medical men, death by hanging is generally induced in one or two ways :—*First*, as the result of the exclusion of air from the lungs, from compression of the trachea, by the cord used in suspension, occasioning asphyxia.—*Second*, by fracture or dislocation of some of the cervical vertebræ, producing certain compression of the medulla oblongata. The latter is unfrequent, except in the cases of corpulent individuals, or when the body suspended receives a rotary impulse, producing in either case a displacement of one or more of the cervical vertebræ, death ensuing rapidly and with little or no suffering. Both the above-mentioned phenomena were noticed in the cases under consideration. The direct cause of death to Williams, the Belgian, was *asphyxia*, or more accurately speaking, *apnœa*, produced by the pressure of the rope upon the air passage. The rope was so adjusted, in this case, as on tightening, to compress the neck *below* what is known to anatomists as the cricoid cartilage, bringing the strain directly upon the trachea, and thus

excluding the air from the lungs. The phenomenon of dissolution was the same as that noted in cases of suffocation, by falling in of earth, in excavations—such as most physicians have seen among laborers on railroads, etc.,—by drowning; by the obstruction of the air passages from imperfectly masticated food; or, as frequently occurs in children, by an abnormal deposit in the larynx or trachea, as in croup, etc. Thus the compression or exclusion of the respiratory tube, preventing the aeration or oxygenation of the blood, is the direct cause of the cessation of circulation, through the minute vessels of the lungs, and the action of the heart is suspended from a deficiency of vital stimulus, occasioned by diminished circulation.

Williams was a man who would be characterized, by those dealing in that species of nomenclature, as of nervous, sanguine temperament. I understand from a phrenologist of some eminence, that his *reverence*, *ideality* and *hope* were largely developed, while his *combativeness* and *causality* were correspondingly small. The character which this gentleman deduced from his phrenological *developments*, (I think that is the term,) was that of an inoffensive man, little given to animal or emotional ebulition, but pre-eminently fixed in purpose. I introduce these observations merely as a matter of general interest, not offering an opinion as to their value. On the scaffold, Williams displayed much agitation, and a muscular tremor was perceptible to every one in view. His pulse indicated 158 beats per minute. His voice was quite clear, although tremulous.

In the case of Cox, the negro, the fatal result was due to the occurrence of the second class of phenomena previously noted. The adjustment of the rope was precisely identical with that in the other instance, but a convulsive movement of the body, or some other cause, at the moment the drop fell, imparted a slight rotary motion to the body, so that the knot slipped forward until it rested just beneath the angle of the under jaw. The trachea was not compressed at all, and on loosening the rope, when the bodies were taken down, there was no rush of air into the lungs, as was noticed in the case of Williams. Here the cause of death was unquestionably fracture or dislocation of the vertebral column, and compression of the spinal cord.

There was little or none of the agitation displayed by the other to be noticed in Cox. No convulsive twitchings of the muscles, or tremor of the voice could be perceived, save by those close at his side. His responses to the petitions offered by the clergymen, were delivered in a firm and audible voice. His pulse at no time indicated above 104 beats per minute.

After the drop fell, four minutes intervened before the cords which bound the arms were loosened. During this interval there was no indication of pain from either individual. Nothing of the gasping, shrugging of the shoulders or writhing of the limbs, and the contortions which have not unfrequently been noticed in executions, was present.

During the fifth minute, Williams' pulse indicated 70—Cox's 36; during the 6th, Williams' 67—Cox's 23; 7th, Williams' 60—first half of the seventh minute, Cox's pulse gave 9 beats, latter half, it was not perceptible. During the first half of the eighth minute, 19 pulsations were faintly distinguishable in the wrist of Williams; but no throb could be perceived during the latter half. In Cox's wrist no pulsations could be detected after the first half of the seventh minute, and the cessation of the heart sounds was almost simultaneous. The heart of Williams continued to beat audibly throughout the tenth, eleventh and twelfth minutes, after which no pulsation could be detected either at the heart, or over the aorta .At the seventeenth minute a faint flutter was revealed through the stethoscope, but it was regarded as merely a muscular contraction. In both cases the extremities were cold at the fifteenth minute.

At the end of thirty minutes the bodies were lowered from their position and placed in coffins. On relaxing the rope around Williams' neck, there was a rush of air into the lungs, plainly indicating that strangulation was the cause of death. This phenomenon was not observed in the case of Cox, the respiratory passage having remained unobstructed. The countenances of both presented a similar, and indeed an identical appearance. There was turgididity and suffusion of the face, which extended to the shoulders, arms and chest. The eyes were open and projecting, and their vessels much injected. The

features were distorted and the tongue protruding. The main points of dissimilarity were such as would be anticipated in persons of different color.

I may here express my obligations to Drs. Garcelon and Fessenden, of Lewiston, for their assistance in remarking the various phenomena, and to Dr. Cummings, of Portland, for his very accurate notes which have aided me in preserving their history.

Time and space allowed me in the Reporter forbid my enlarging upon this interesting subject. I cannot, however, omit a reference to a fact which opens a question for the decision of physiologists. In the case of both the culprits executed at Auburn, a circumstance occured, which is said invariably accompany death by hanging, viz:—an erection of the penis, and an *emissio semenis.* The direct connection between the nervous branches implicated in suspension by the neck, and those distributed to the organs of generation, has not that I am aware, yet been demonstrated.—*Maine Medical and Surgical Reporter.*

EPILEPTIC CONVULSIONS DEPENDENT UPON LESIONS OF THE SPINAL CORD.

By Brown-Sequard.

(Translated from Archives Generale.)

M. Brown-Sequard has determined that certain lesions of the spinal cord are followed, after the lapse of several weeks, of epileptiform convulsions, coming on by paroxysms, but which can be produced at will by irritating the skin of portions of the face and neck. Believing these convulsions similar to epilepsy and the peripheral irritation producing *aura epileptico*, he could never identify them.

He now believes that in many cases the sensibility is not only abolished, but the reflex movements are not produced during the paroxysms. He concludes that this convulsive affection is nothing more nor less than epilepsy.

Besides this M. Brown-Sequard has determined that animals, with a complete or almost complete section of the spinal chord in

the dorsal region, have convulsions of *spinal epilepsy*, entirely different from disease of the same name described by J. Frank, Wunderlich, Copland, etc. He says that convulsions in true spinal epilepsy, only exist in the paralysed parts, and not in those which receive nerves from the encephalon or cervical portion of the spinal cord.

The same convulsive paroxysms are seen in man in those parts which receive nerves above the point of disease of the spinal cord. This lesion of the spinal cord is most frequently dependent upon a *ramorllisement* and the convulsions are dependent upon modifications of the cord itself, exhibiting its reflex function, or upon an incitation of the skin or the mucous membranes. The latter is the most frequent.

These convulsions present varied forms, but are easily distinguished from simple reflex movements. They may be prolonged, as in inferior animals, during a quarter of an hour. Simple repose in paraplegia, where nutrition goes on, may induce a considerable increase in the reflex power. It may be possible that in some cases of epilepsy not accompanied with paralysis, the disease may not have a limited seat, as is generally the case, in the spinal cord, and in the cerebro-spinal axis, governing it; but may be in other portions of the spinal cord.

The author refers to the contraction of the cerebral blood vessels, during epileptic attacks, and the circulation of black blood after this contraction has ceased. The contraction of the blood vessels themselves is under the influence of the spinal cord or its appendages. The great sympathetic plays an important part as an agent of transmission.

M. Brown-Sequard has experimentally proven the contraction of the cerebral vessels at the commencement of attacks in epileptic animals, and he has noticed its absence after the section of cervical sympathetic—*Savannah Jour. of Med.*

A NEW METHOD OF TREATING ASPHYXIA.

To the Editor of the Lancet: Sir—Your correspondent from Columbia, Tennessee, U. S., appears to be much interested in the method of treatment in asphyxia recommended by the late Dr. Marshall Hall, and generally known as the "Marshall Hall" or "Postural Method." Allow me, through the medium of your journal, to call his attention to the results of my experiments and observations on this important subject.

The principal involved in the postural method is precisely the same as in a plan formerly much practised, in which simple compression and relaxation of the chest were employed to induce expiration and inspiration. The difference is this, that in the postural method the compression of the thorax is occasioned by the weight of the body of the patient resting on the chest during the prone position, followed by the removal of the pressure and consequent return of the thoracic parietes to their ordinary level on the patient assuming the supine posture. No attempt is made to enlarge the actual capacity of the chest by inducing an elevation of the ribs, such as takes place on making a deep inspiration.

This method of treatment has been found open to some objections; for instance—

1. Expiration is made to precede inspiration, whereas originally expiration is second in order, and not the primary act. In still-born infants, whose lungs have never been inflated, forced expiration at first is of course impossible.

2. The expansion of the thorax or inspiration, being dependent on little more than the elasticity of the tissues, takes place feebly, inefficiently, and therefore calls for some more active mechanical aid.

3. It is scarcely possible to use the warm-bath, if indicated, in the case of a still-born infant, during the adoption of the postural method.

4. The patient is liable to have the mouth and nose compressed, the face bruised, or the neck twisted, by the almost lifeless body being turned alternately on the chest and back fifteen times a minute for some hours; moreover to the operator this process is very arduous.

5. When the body is "turned on the face, and pressure made on the back" (pronated), the contents of the stomach are liable to pass into the œsophagus and windpipe.

6. When the patient is turned "completely on the side and a little beyond" (supinated), the tongue obstructs inspiration by falling back into the throat, the epiglottis resting against the back of the pharynx.

7. Both sides of the chest are not equally inflated, one side only being called into action at the same time to any important extent.

8. This process is not entirely in harmony with that of nature. It is not the way in which we generally breathe.

9. That the amount of air respired is exceedingly small as has been proved by actual experiment. This is doubtless in consequence of the actual capacity of the chest not being increased. The method of performing the experiments, at St. George's Hospital, on the dead subject, which appeared to prove that in the Marshall Hall Method "nearly as much air entered the lungs as would be inhaled in an ordinary inspiration in a state of health," is, I believe, by no means satisfactory; it is, in fact, open to obvious objections. The tube of the pnæometer was passed into one of the nostrils of the patient, the other nostril and the lips being closed with adhesive plaster. The want of rigidity of the cheeks, and the amount of air in the respiratory tract, and even in the stomach, etc., of the patient, could scarcely fail to render the indications of the instrument, however perfect in itself, liable to suspicion, if not entirely valueless, in point of scientific accuracy.

The method which I have ventured to bring before the profession, is not open to any of the above objections. It is a simple imitation of natural deep respiration, and effected by means of the same muscles as are employed by nature in that process. In ordinary deep inspiration, we lift the ribs and sternum by the pectoral and other muscles, which pass between the chest and the shoulders, and thus produce the threatened vacuum which inflates the lungs.

In my method we lift the ribs and sternum by the pectoral and other muscles which pass from the shoulders to the parietes of the thorax, by steadily extending the arms up by the sides of the patient's head; by elevating the ribs, the cavity of the chest is enlarged, a tendency to a vacuum is produced, and a rush of air immediately takes place into the lungs. Expiration is brought about by simple compression of the sides of the chest by the patient's arms.

The distinguished feature of my method is the actual enlargement of the cavity of the chest.

In order to maintain a free entrance of air into the windpipe, we draw forward the patient's tongue, and keep it projecting beyond the lips. If the lower jaw be gently raised, the teeth may be made to hold the tongue in the required position. Should it be found necessary, the tongue may be so retained by passing a handkerchief under the chin and fastening it over the head.

In this way the patulous orifice of the windpipe is raised and drawn forward, so that nothing intervenes between it and the natural channel of air through the nose. The tongue is entirely prevented from falling back into the throat, whilst the extent to which the windpipe put on the stretch is clearly indicated; the pharynx also is sufficiently opened to allow of the removal of liquids, etc., from the mouth, nose, pharynx, etc.

My method has been tested by experiment on the dead body with most satisfactory results; and a successful case of resuscitation has already been recorded.

Yours obediently,

HENRY R. SILVESTER, B.A., M.D., London.

High street, Clapham, 1858.

MIDWIFERY.

MENSTRUATION AND CONCEPTION.

By Professor Bischoff.

THE attention and laborious investigation of the student has been long directed towards that most interesting and important point, the physiology of menstruation and conception, and

amongst the many who have labored in this field, we know of none more distinguished than Professor Bischoff. We design to give our readers an abridged sketch of his recent labors, and the results to which he has arrived.

M. Bischoff gives in detail his examination of the organs of generation in thirteen women, who had died suddenly whilst menstruating; the death, in most of the instances, being the result of accident or violence. These examinations were made with the greatest accuracy; and, when taken in connection with the previous history of each case, will aid very much in determining the physiological facts involved. Our space forbids us from presenting the minute details of each dissection, and we will therefore only give the deductions derived from them.

Professor Bischoff remarks that these cases confirm the doctrine that in women, at every menstruation, a follicle ripens, swells and bursts; that the ovum escapes, and that a corpus luteum is formed. The eleventh case seems to prove that the full consequences of menstruation are not in every instance necessarily carried out, but that a follicle may swell, and the ovum ripen without the bursting of the follicle, or the escape of the ovum. Such a condition will cause sterility, notwithstanding menstruation. Perhaps, also, the pain of the menstrual period depends upon this incomplete development.

A woman may not menstruate, and yet she may conceive; for the essential condition, the ripening and escape of an ovum, may proceed, and only the usual outward symptoms of this event, the secretion of blood, fail. Professor Bischoff remarks that he could never suceeed in finding the ovum, but observes that he does not believe that it is so soon destroyed. He says there are but two cases known to him where it was so found, and that only one of these presents the desired certainty. This is the following observation of Professor Hyrtl, of Vienna:—Therese Michal, aged 17, died on the 10th of October, 1844, of peritonitis, in the clinique of Prof. Oppolzer. She had only menstruated twice in her life, and the last time was two days before her admission into the hospital, on the 8th October. The breasts were tolerably developed; the pubes slightly covered with hair. The hymen was uninjured; the uterus of moderate size, compact, its cavity

filled with a considerable quantity of thick blood; the mucous membrane was so loosened as to resemble a half-liquid plastic exudation. The lining membrane of both Fallopian tubes was suffused, loose, and covered with mucus. Both ovaries were of a fair size, but the left one showed a ruptured follicle of the size of a large hazel-nut, filled with semi-fluid blood. Prof. Hyrtl submitted the parts to careful inspection under the microscope on the same day. Neither in the vagina, uterus, or tubes could he find a trace of spermatozoa. But by a careful examination of the left tube, in that part which traverses the substance of the uterus, he found an ovulum, with all its characteristic properties, somewhat dull and turbid indeed, but fully recognisable. Hyrtl believes he even detected the germinal vesicle. This is a remarkable anomally, since the germinal vesicle of every ovum, when it has left the ovary, has disappeared. Prof. Hyrtl had the ovulum immediately drawn by his artist, and exhibited it on the same day in his lecture on physiology. This case is free from doubt, as there is no room to suspect coitus. Prof. Bischoff regrets that he cannot admit as much with reference to the cases of Dr. Letheby, published in the "Philosophical Transactions" for 1852, but his reasons of dissent do not appear to be very valid.

Prof. Bischoff takes occasion to discuss the question as to the differences between the corpus luteum of simple menstruation and that of conception. In the cow and sow the corpora lutea of the non-pregnant are altogether similar to those of the pregnant, and he has made the same observation in the bitch, the cat, and the porpoise. They disappear more quickly when no conception follows. In the human female, at the earliest period of the bursting of the follicle, the filling of it with blood, and the beginning of the development of the membrane of the follicle, to the formation of the corpus luteum, there is no difference at all. But in the further development of the corpus luteum, the impregnation of the ovum and gestation bring about a material difference. The corpus luteum in simple menstruation never attains the full stage of formation up to the complete filling up of the follicle and the metamorphosis of the blood coagulum; but this substance only developes itself into a peripheral layer, and soon falls into retrogressive metamorphosis. It shrivels up, and only

leaves at the next and the succeeding menstruation a more and more indistinct spot, changing from yellow to brown and black, and soon there remains nothing but a scar, which lasts for a time, and gives evidence of the past process which has at an earlier period taken place on the surface of the ovary. The corpus lutea of conception, on the other hand, goes on developing itself throughout the first months of pregnancy, and attains a fullness of size, color and texture, which the menstrual corpora lutea never reach. It lasts throughout the whole period of pregnancy, and disappears after delivery. It may however be questioned whether it has a great diagnostic value. In the first period, so long as as there are no other signs of pregnancy, these differences either do not exist, or are very difficult to determine. After delivery, when the signs of pregnancy in the uterus have disappeared, it again becomes difficult to distinguish them from the corpora lutea of fourteen days or three weeks' standing, resulting from menstruation. And where pregnancy is plain, from the state of the uterus and other parts, the corpus luteum is of no value. Prof. Bischoff, therefore, concludes that the corpus luteum cannot be used in the determination *of doubtful cases.*

Professor Bischoff holds it to be proved that conception and pregnancy depend absolutely upon menstruation as the period of the ripening and expulsion of an ovum. He remarks, however, that it can scarcely be doubted that the time occupied by the transit of the ovum from the ovary, the arrest of the ovum, and the preparatory changes in the uterine mucous membrane, suffer great differences in individuals, and hence entail a longer or shorter duration of gestation.—*Henle's Zeitschrift, and Monthly Jour. Med. Science.*

ON THE CONTRACTIONS OF THE UTERUS.

By John Christie, M. D., Lecturer on the Institutes of Medicine, University and Kings College, Aberdeen.

In describing the characters of a true labor pain, Dr. Rigby says, that "every pain which commences in the fundus (uteri) is abnormal," and declares, on the authority of Wigand, that, "in

examining the course of a true pain, we shall find that the contractions of the uterus do not begin in the fundus, but in the os uteri, and pass from the one to the other." He further observes, that "we very seldom find that a contraction of the uterus, which has commenced in the fundus passes into the cervix and os uteri, and becomes a genuine effective pain; usually speaking, the contraction is confined to the circumference of the fundus, without detruding the fœtus at all." Following these general statements, we have a minute description of the phenomena and progress of what he calls a true pain. The common belief is, that, on the commencement of a pain, the head is forced down closer to or into contact with the os uteri; but, according to the views he borrows from Wigand, "so far from the head being pressed against the os uteri, it at first rises upwards, and sometimes gets even out of the reach of the finger, whilst the os uteri itself is filled with the bladder of membranes." "If it had commenced in the fundus," he argues, "instead of in the inferior segment of the uterus, so far from the head being drawn up at the first coming on of the pain, it would have been forcibly pushed down against the os uteri." A pain according to this to be genuine, must be marked by the following characters:— "First the os uteri grows tight, and the presenting part rises somewhat from it; then the rest of the uterus, especially the fundus, becoming hard, the patient has a sensation of pain, and the presenting part of the child advances."—*System of Midwifery.* London, 1841. P. 99.

Dr. Churchill also seems to subscribe to the same doctrine, since he observes, "The contractions commence in the cervix, according to Müller, Michaelis, and Wigand."—*Theory and Practice of Midwifery.* Third Edition. London, 1855. P. 176.

Dr. W. T. Smith, the most recent systematic writer on obstetrics, believes the "view of Wigand, which has been particularly insisted upon by Dr. Rigby, to be a very accurate description of the direction in which the uterus contracts during a labor pain."—*A Manual of Obstetrics.* London, 1857. P. 201. Dr. Smith refers to various facts noticed with regard to the direction of peristaltic action in different organs of the body, and infers from them the probability that the contractile action "of the uterus does take this course" from below upwards.

Dr. Burns does not say anything specific on the subject; nor do his great precursors, Drs. Smellie and Denman. These authors, in fact, never seem to have surmised that the received opinion as to the uterus acting from above downwards, could be called in question. Dr. F. Ramsbotham also appears to agree with the common view; but in any question involving the physiology of the parturient process, no great weight is attachable to his opinions one way or another, since he neither seems to understand the dynamical phenomena of parturition, nor the mechanical relations of the fœtus and maternal pelvis, which constitute the mechanism of the process.

It would be easy to multiply references to this subject; but my purpose is not to elucidate its history. It is its practical import merely with which I mean to deal. Now, I am bound to say, that anything more erroneous than the views of Wigand and others, as adopted by Rigby, Churchill, and Smith, can hardly be conceived. They are in direct contradiction to the phenomena to be observed in every case of natural labor. For many years past, I have rarely omitted to make observations in regard to this matter, and in no instance have I ever been able to detect anything of the peristaltic motion described by the authors in question. In fact, I doubt greatly if the action of the uterus in labor is of the kind that can, strictly speaking, be called peristaltic. By this kind of motion we understand contraction beginning at a particular point, and passing onwards as it were in a progressive wave, the parts in which it commences, and those over which it spreads, being relaxed as the contraction moves from one point to another; but nothing analagous to this takes place in the uterus during labor. The contraction, it is true begins in the fundus, and extends thence downwards over the body and cervix of the organ; but one point is not relaxed as the contigious one begins to contract. From the first to the last moment of contraction the fundus is active. Dr. Tyler Smith contends that, since the the uterus unquestionably does contract peristaltically under certain circumstances, this kind of action must necessarily be "the basis of the other uterine actions." And misled by the statements of Wigand, he thinks he finds confirmation of his views as to what takes place in labor, viz., that uterine contrac-

tion begins in the cervix, and spreads from that part over the body and onwards to the fundus. But without denying the peristaltic motion of the uterus in certain circumstances, it is very difficult to find any trace of it in the contractions of the organ in natural labor; and the phenomena observed, and most correctly recorded by Wigand, certainly do not countenance the inferences deduced from them when they are rightly interpreted.

Wigand, and those who follow him, consider that the contractions begin in the os uteri, and, as a consequence, they describe the head of the fœtus as being forced upwards, sometimes "even out of the reach of the finger." When the contraction reaches the fundus, according to them, it of course takes a contrary direction, and this is marked by the descent of the head and its propulsion against the os uteri.

As already observed, although the correctness of the description given of this part of the parturient process by Wigand is admitted, the deductions made from it are denied. Let us then see how the matter can be explained. Very numerous and carefully made observations have satisfied me that the actions of the uterus, constituting a true pain set in thus:—*First*, The fundus of the uterus contracts; *second*, The contraction, while each instant becoming stronger in the fundus, passes into the body of the uterus; and, *third*, Terminates by becoming manifest in the cervix. Coincidently with the beginning contraction of the fundus uteri, the bag of membranes begins to be distended; and this, wrongly construed, it is which has led Wigand and others to imagine that the head "at first rises upwards." I am firmly persuaded that no such displacement of the head occurs. Moreover, I hold that it cannot occur, for the os uteri does not contract antecedently to the fundus. I speak within the mark, when I say that I have a hundred times watched the progress of a pain, with a view of elucidating the point in question, with the fore-finger of the examining hand in the os uteri, while the other hand was spread over the patient's abdomen. Very often the first indication of the commencement of a pain is found to be a slight increase in the volume of the bag of membranes, and this while the os uteri is undoubtedly in a relaxed condition. Immediately after, or as near as may be at the same instant, the uterus is felt, by the hand

spread out on the abdomen, beginning to contract at its fundus and the whole uterus is soon involved in the action. As the elder Nægele so well expresses it, "the contractions are of that kind, that the uterus contracts most strongly at its fundus, and in the body stronger than in the inferior segment. In this manner the os uteri is dilated, and that which is contained in the cavity of the uterus is forced towards it and expelled."

Dr. Murphy also describes the contractions of the uterus as taking place from above downwards. "This," he says, "may be ascertained experimentally. For instance, when the hand is passed into the uterus, after delivery, to remove the placenta, we find that it may remain for some time in the cavity without exciting its contraction; but the moment the hand is withdrawn, the fundus instantly contracts, and as it passes along the vagina, the contractions are continued from above downwards. So also, in other instances, when the os uteri is only irritated by the fingers of the hand introduced into the vagina, and an attempt is made to dilate it, the fundus immediately contracts, not the os uteri."—*Lectures on the Principles and Practice of Midwifery.* London, 1852. P. 51.

It follows from this, that Wigand's view is erroneous. The apparent rising upwards of the head is not due, as he explains it to the contraction of the os uteri forcing it, in the first instance, towards the fundus, but to the flow downwards of the liquor amnii when the fundus begins to contract; and the fluid accumulating in the presenting bag of membranes, causes the apparent displacement of the fœtal head upwards. The rising upwards of the head is, therefore, not a real phenomenon. It is wholly due to the accumulation of fluid between it and the membranes; and the matter has been so often investigated and re-investigated, in the course of my own practice, that I feel no misgiving in insisting on that just stated as the true explanation of it.

Dr. Murphy admits that the head retires upwards, although he contends that the uterine contractions begin in the fundus uteri, and are propagated thence downwards throughout the other parts of the uterus. "In Wigand's explanation," he says, "the influence of fluid pressure seems to be altogether forgotten. The immediateeffect of contraction commencing at the fundus, would

be to to compress the liquor amnii, which of necessity forces its way before the head, on to the mouth of the uterus. The fluid in this position reacts against the head with the same power that it is compressed, and therefore pushes it up until the increasing contraction of the fundus forces the head down again" (pp. 52–3.) This is a very ingenious, but not a true solution of the case. If the fœtus, at the full term of pregnancy, were floating free in the liquor amnii, I could easily understand how things might happen exactly as thus supposed by Dr. Murphy. But this it does not. When the fœtus is so placed in the uterus, that its long axis corresponds with the long axis of that organ, the part of it which is turned towards, is generally in contact with the fundus. Moreover, in proportion as the fluid contents of the uterus descend, so also do the dimensions of its cavity become less, and the more closely do its walls and the body of the contained fœtus come in contact. Further, as Dr. T. Smith remarks, during the progress of pregnancy, "the relative quantity of the liquor amnii diminishes, so that at the full term the liquor amnii scarcely does more than fill up the interstices left between the fœtus and the uterus" (p. 217). And in many cases it exists in very small quantity. According to Osiander, it may scarcely measure a table-spoonful. It is very rare, however, that it exists in so small a quantity; but where it has been very much below the average, which is commonly between one and two pounds, and where the breech of the fœtus has been felt in close contact with the fundus of the uterus, I have found the bag of membranes, and the apparent recession of the head present themselves as where the liquor amnii existed in a very much larger proportion. I am therefore constrained to reject even Dr. Murphy's most ingenious, but purely speculative, explanation of how the fœtal head comes apparently to recede from the os uteri in the beginning of a pain.

Scanzoni points out that, if the contractions in the beginning of a pain were "only limited to the lowermost segment of the uterus, the bag of membranes would also, at the same time, be necessarily forced upwards; but that he never could perceive this. On the contrary, indeed, we find that it is only forced so much the more deeply, the farther the presenting part of the child recedes from the finger." Scanzoni also maintains "neither in the

fundus nor in the lower segment of the uterus, does a normal pain begin; but it is equally spread over the whole uterine substance, and must be so, if its object is to be realized." Braun adopts a similar view. "In a physiological pain," he says, "the contractions of the uterus do not begin at the os uteri, but at the orifices of the Fallopian tubes, and thence spread equably over the whole circumference of the uterus and develops its centripetal action, which in all other hollow muscles is towards the mouth, in the direction of its orifice.

It is manifest from all this, that Wigands view of what occurs during the early contractions of the uterus, is essentially erroneous. Nor does Dr. Tyler Smith, who treats of this matter with a great degree of apparent scientific exactness, succeed in giving to it anything like efficient support. He quite loses himself in the maze of reflex and peristaltic action, which comes crowding in upon him when he sets himself to give an account of the phenomena of uterine contraction during labor. Evidently misled by Magendie's account of the motions of the stomach, he strives to establish an analogy in the case of the uterine contractions. But if he had taken the heart, the intestinal canal below the stomach, or the bladder, he would have at once seen that his analogy was a false one. Brought, then, to the test of observation, and tried by what actually occurs in nature, we see that in normal labor, the os uteri does not contract at the outset of a pain, and that from this point the action does not spread upwards to the fundus, return downwards, and terminate at the place where it began. To talk of the fœtus being expelled by the peristaltic action of the uterus is absurd; contrary, in fact, to all the observed phenomena of normal uterine contraction, and, indeed, impossible.—*Edinburgh Med. Journal.*

THE PLACENTIAL SITE AFTER DELIVERY.

By Robin.

THE internal surface of the uterus, at the spot from which the placenta has been detached, retains the serotina or mucous membrane of the part, except the thin superficial layer which has been detached and carried away by the placenta.

Between the serotina and the muscular layer of the uterus there is not produced a new mucous membrane, or membrane to replace the old one, as happens between the caduca uterina and the adjacent muscular layer.

It is only during the continuation of the lochia that the serotina exfoliates and eliminates itself. Then only is it replaced by the continuation of the thin mucous membrane for replacement, in proportion as it exfoliates.

The contraction of the uterine walls diminshes greatly the extent of the surface of the serotina. It is reduced to a breadth of about 6 to 8 centimetres, and this diameter goes on always diminishing. The membrane thus gains in thickness what it loses in breadth by this contraction. At the same time, its surface becomes folded, ruguous as if mammillary; its tissue becomes brownish or reddish, it softens and has a surface of a mucous or pultaceous consistence.

The borders of the serotina are continuous with the thin, new mucous membrane which covers the rest of the uterus. The serotina is pultaceous or mucous, softened, easy to remove by scraping; its color is reddish-brown or greyish.

It is not rare to see, on the surface of this layer, vascular orifices closed by reddish brown or slightly decolorized fibrinous clots. Following these clots by dissection, we are often led as far as the sinuses in the muscular tissue of the uterus—*Journal de la Physiologie, Janvier.*

THERAPEUTICAL RECORD.

Ague.—Von Holbeck is accustomed to treat Ague as follows: Infuse an ounce of well-roasted coffee in three ounces of boiled water, strain, acidulate with lemon-juice. The whole to be taken at once, five hours before the expected ague.

Face Wash.—Mr. Vonoge, of Paris, has found that in those cases of red spots or efflorescence of the face so often seen in young people, that washing them several times a day with Hufeland's formula, a most excellent remedy. It consists of

Borax, two parts,
Orange flower, and
Rose water, of each fifteen parts.

Chilblains.—Dr. Turnbull has found this of service in chilblains, by saturating a sponge or flannel in the concentrated tincture of capsicum, and rub the parts until a strong tingling or electrical feeling is produced. He says it never produces excoriation, unless the skin be broken. He also has used it in toothache, by putting a drop or two of the tincture on some cotton, and then apply to the part affected. The relief will be immediate.

Chloride of Iron.—1. As a local or external hæmostatic, 3 to 5 parts chloride of iron to 100 parts of distilled water. Lint soaked in this mixture is to be applied with more or less pressure on the seat of the hemorrhage.

2. As an internal hæmostatic, 1 part of chloride of iron to 500 of distilled water, sweetened to taste. One tablespoonful to be given every hour, or oftener if necessary. This formula suffices to check the fiercest hemorrhage within 24 hours. The same formula, without sugar, forms a useful uterine injection or astringent lavement in cholera or colliquative diarrhœa.

3. A hæmostatic and resolvent ointment is composed of 4 to 15 parts of chloride of iron to 30 of axunge. With this V. says he has cured an indolent ulcer of twelve years' standing, a tumor albus of four years' standing, several cases of rheumatism and arthritis, and many polypous, warty, and condylomatous growths about the anus.—*Jour. de Med.*

Umbilical Hernia.—A child, six weeks old, after a violent fit of crying, was found affected with an umbilical hernia. M. painted the swelling with collodion; and so soon as this had dried, and so drawn the skin together, the hernia disappeared and did not return. The swelling was the size of a hazel-nut, the part painted was that of a crown piece. The collodion must be pure; no previous reduction of the hernia is necessary; should the circumference become red and wrinkled, a little glycerine rubbed round the edge will dissolve it and set the constriction free. The layer of collodion falls off after from seven to eight days, and may then be renewed. With moderate care on the part of the nurse, the child may be bathed as usual. The French physician Pradier has also successfully employed this method in the case of a child one month old.—*Gaz. Hebd.*

Sugar.—Sugar has been found in the urine of parturient females, nurses, and such pregnant females as have their mammæ sympathetically enlarged, by Blot, chief of the maternity clinic at Paris. This was ascertained by chemical reactions by the formation of alcohol, and by the diversion of the solar ray to the right. The quantity of sugar found was about 1–12 grm. to 1,000 grms. of urine. The sugar seemed to depend on the coexistent secretion of milk, the quantity found being always proportionate to the health of the individual and the amount of milk secreted. This phenomenon was also confirmed by the examination of the urine of nine milk-cows.—*Gaz. Hebd.*

Ptyalism of Pregnancy.—In the ptyalism of pregnancy, after the failure of ordinary astringent means, Lamæstre succeeded in obtaining a cure in a few days by the use of pastilles (lozenges), containing five centigr. (about half a grain) each of iodide of potassium. Four or five of these were used at regular intervals during the day.—*L'Union Medicale.*

Post-partum Hemorrhage.—In a case of post-partum hemorrhage, which had resisted all other means, Dupierris employed successfully a uterine injection of one part T. Iodin. to two of water, there were no bad results; the patient convalesced favorably. The same injection was also found extremely useful where a too copious lochial discharge is consequent on a relaxed state of the uterus.—*Gaz. d. Hop.*

Placenta.—The *Pacific Medical and Surgical Journal* says:

"Dr. A. Legrand (*Gazette de Hopitaux* of various dates) asserts that cold water injected into the umbilical vein is efficacious in detaching the placenta in abnormal adhesions, and in dangerous hemorrhages, immediately after accouchement. It is a very simple thing to perform, and can do no harm, though, in some rare cases of adhesion, in is entirely inadequate."

Incontinence of Urine.—Allusion has already been made to the successful use of belladonna in incontinence of urine in children, and we now refer to the subject merely to say that subsequent experience seems to confirm the good opinion at first expressed

of it. Mr. Hutchinson, of the Metropolitan free hospital, reports a very obstinate case entirely relieved by it. At first the belladonna seemed to have no effect; but being persisted in until symptoms of poisoning were manifested, its good effects were experienced.

Ununited Fractures.—Professor Brainard reports, in the September number of the Chicago Medical Journal, thirteen cases of ununited fractures, treated according to his new method by subcutaneous perforation of the bone. It will be remembered that Professor B. obtained the prize of the American Medical Association for 1854, for an essay containing an account of this method of bringing about union in fractures where it fails to take place spontaneously. The cases now reported have been healed on this plan since the publication of his essay. This report is of interest to surgeons, as in it he points out the advantages and defects of his method of operating, and also suggests some improvements in the manner of its performance.

Uterine Polypus.—Dr. W. Channing has found in certain cases of intra uterine polypus, that the structure is destroyed by a ligation embracing only a portion of the growth. "In one case he was desired by Dr. York to see a case with him in consultation. The tumor was found (as previously described) and it was agreed that a ligature should be put around it. This was done, but as the tumor tapered towards its lower end, the ligature slipped until it rested, as it seemed near that end. It was drawn, and the strain being continued, it cut itself out, bringing away a section of the tumor of the diameter and thickness of a common coat button. Hœmorrhage ceased at once, and never returned. The tumor became smaller and smaller and soon disappeared, when general and perfect health was established. This case is in support of the views of Gooch and the best authorities, that if any, however small a portion of the polypus be cut off, the remainder will entirely disappear, just as the whole of the navel string will come away where ever the ligature may be applied." —*Boston Med. and Surg. Jour.*

Dysmenorrhœa.—Dr. W. O. Barker, of Omega, Texas, gives the following plan for the treatment of dysmenorrhœa, which we

take from the New Orleans *Med. News and Hos. Gaz.* for Nov., 1858. "From the nature and cause of the disease, I was induced to believe that it partook strongly of the nature of rheumatism. I therefore prepared the following:

℞ Gum Guiac, - - - ℥i.
Potassa nitr., - - - ℥i.
Flor. Sulphur, - - - ℥i.

M. All to be well ground in a mortar, and put into one pint of brandy or good whiskey, and after standing a few days, I directed it to be taken in tablespoonfull doses morning and night, commencing one week before the menstrual period. A perfect cure has been the result of each trial with the above remedy. In a very few cases the remedy had to be continued.

Dropsy.—The following has been recommended in dropsy:

℞ Potassæ Carb. - - - ℥i.
Infus. Gentianæ Comp. - - f℥ ix.
Sp. Ætheris Comp. - - f℥ iij.
Tinct. Cinam. Comp. - - f℥ v.

M. A tablespoonful every three hours.

Chloride of Iron.—In a communication to the French Academy, detailing the results of his experiments, Delan says:

1. The chloride of iron is not dangerous either in its internal or external application.

2. It is the most powerful of all known hæmostatics.

3. It modifies living tissue, and, in virtue of this action, is specially applicable to the treatment of blennorrhea, leucorrhea, bronchial catarrh, etc.

4. It is anti-syphilitic, inasmuch as it heals chancres and venereal ulcerations of the vagina and uterus, without any of those dangerous results which sometimes follow the use of nitrate of silver, iodine, mercury, and its combinations.

5. It is a powerful remedy in scrofula.—*Gaz. Med.*

Anti-galactic.—Professor Rousset says that enlargement of the galactophorous tubes can be cured by iodide of potassium. He gives it in small doses daily until the effect has been produced.

Luxation of Humerus.—Dr. Smith gives the following as a new method of reducing luxations of the shoulder:

To reduce a luxation of the head of the humerus into the axilla, etherize the patient; then carry the elbow about 45 degrees from the side of the body, and flex the fore-arm to a right angle with the arm, so that the palm of the hand presents to the patient's abdomen; then using the fore-arm as a lever, rotate the head of the humerus forward and upward by making the hand describe a semi-circle from before backward, until the palm of the hand looks up, the elbow being kept off from the side; then holding the fore-arm in its semi-flexed position, with the palm looking to the operator, carry the elbow gently into the side, and quickly—but without force—rotate the head of the humerus backward and upward by reversing the motion of the fore-arm, so as to cause the hand to describe an entire circle. When this manipulation is well performed, the reduction is prompt and very easy, especially when the scapula is steadied by the hand of an assistant.

In the anterior or posterior luxations, carry the arm as nearly perpendicularly upward as is necessary, or in such a position as will throw the head of the humerus into the axilla, and proceed as before.

Syphilitic Vegetations.—M. Hairion, after describing the advantages derivable from the chromic acid in certain forms of the granular eyelid (a disease of common occurrence in the Belgian army), observes that the trials he has made of the acid, as recommended by Marshall and Heller in syphilitic vegetation, have been attended with the most complete and rapid success. Moreover, its application, whether to these syphilitic vegetations or to the fungous granulations of the conjunctiva, is never attended with pain or reaction, notwithstanding the rapid destruction of tissue that takes place.—*Annales d' Occulistique.*

Croup.—Dr. Pudon relates some cases as examples of the great benefit he has derived from the continuous application of cold wet compresses to the neck, simultaneously with the administration of sulphate of copper, in two-grain doses every half hour; sixty-four grains having been given in one case, and seventy in another.—*Journal fur Kinderkrank.*

Herpes Zona.—Professor Fenger has of late been treating this troublesome affection advantageously by collodion, smearing it by means of a pencil over the whole of the vesicles, their basis and their circumference, or wherever there is redness. It should be applied as early as possible, and three layers in thickness, renewing it next day. He finds the addition of castor oil to the collodion an improvement; but especially prefers the solution of cotton wool in acetic ether.—*Schmidt's Jahrb.*

Neuralgia.—Dr. Declat warmly recommends valerianate of ammonia in neuralgia, nervous headaches, and hysteria, in doses of one or two fluid drachms, two, three, or four times a day. Other physicians who have used it in these affections speak highly of it. It may be prepared as follows: Take of distilled water four fluid ounces; valerianic acid one drachm; carbonate of ammonia, a sufficient quantity to neutralize the acid; then add of alcoholic extract of valerian, two scruples.

Inflamed Nipples During Lactation.—In the October No. of the Nashville *Monthly Record*, we find an interesting communication from a lady contributor, giving her happy personal experience in the use of gum arabic as a remedy for the relief of sore nipples. The gum is to be applied either as a "*very thick* mucilage," or in fine powder, and is to be put upon the nipples immediately after nursing, then protect with a piece of soft linen placed over them. She says after the application of this remedy she "never had such relief in her life."

Arsenic in Bismuth.—In the progress of a recent trial for murder, the analytical chemist had occasion to test the purity of bismuth, as that article had been administered by the physician in attendance on the deceased; and small but decided traces of arsenic were found in each of quite a number of specimens examined.

EDITORIALS AND MISCELLANEOUS MATTER.

THE MEDICAL BOARD.

It is with infinite pleasure that we record the passage of an Act creating a Board of Medical Examiners for North Carolina. The Legislature has acted nobly; and in the name of the profesion we congratulate the members of that body on their independence, good sense and liberality in this matter, whilst we return them our kindest and most grateful acknowledgements. They have done well for the State, for humanity, for the cause of science—and their patriotic course will be sustained by the "good and true" from the mountains to the sea shore. A few carping critics or inveterate partizans may close their eyes to the true meaning and effect of this wise and salutary regulation; but in despite of their appeals to the prejudices of the people, and their most contemptible pratings about "class legislation," this measure must be regarded with favor by every man who has sense enough to distinguish between a qualified physician, and a titled ignoramus, or sufficient decency to prefer the association of a gentlemen to that of a quack.

By this Bill the people and the profession are both protected as their best interests require; and we can but anticipate the most decided advantages from its operation, provided its provisions be strictly carried out. To the members of the profession in the Legislature, the friends of the Bill are under many obligations—for it is to their most zealous advocacy of it; their hearty co-operation with the State Society; their deep conviction of the necessity for such a law, that we are indebted for its final triumph. To Dr. Henry, of Bertie, in the Commons, and Drs. Dillard, of Chowan, and Ramsay, of Rowan, in the Senate, the thanks of the profession are particularly due; and whilst we would do ample justice to every physician there who sustained the measure, we feel that it is an imperative duty to express in the most public

and emphatic manner, our high appreciation of the services rendered by these gentlemen especially. We honor them for remembering their profession midst all the excitement and confusion incident to the political arena, and for boldly sustaining the honor and dignity of Medicine without regard to the prejudices of their constituents, and with no heed of the consequences to themselves.

As the State Medical Society has an important part to play in the election of the Members of this Board, its members should see that the task is properly performed. It is to be hoped that there will be no unkind feelings or ungenerous rivalry elicited by this election. Let us select the best men for this responsible position. Let us throw all personal considerations aside and labor with an eye single to the interests of the profession. Let no combinations be formed, no preconcerted arrangements attempted, no electioneering schemes inaugurated; but after having met together in the most fraternal spirit, and having fully and impartially considered the claims of each individual, repudiating all sectional, political and personal prejudices, let us select such of our number for this exalted post as will honor and adorn it. Let us have perfect harmony, complete unity, thorough co-operation, or the scheme will prove a failure as surely as the sun is above us; and the end we so ardently desire can only result from the exhibition on the part of all, of that impartial, unselfish and ingenuous spirit which should distinguish every member of the profession, whatever his attainments, his ambition or his prospects.

We do not appreciate the immediate necessity for this advice; but as human nature is most fallible, and such a contingency is at least possible, it is incumbent upon us, as a public journalist, to sound the note of warning and to point out the lurking danger.

The passage of this Bill in Carolina is a glorious victory; an

event over which every patriot should rejoice; a consummation pregnant with benefits to the profession and to the whole State, and we hail its incorporation into our statute books, with peculiar pride and satisfaction.

AN ACT TO INCORPORATE THE MEDICAL SOCIETY OF THE STATE OF NORTH CAROLINA, AND FOR THE ESTABLISHMENT OF A MEDICAL BOARD OF EXAMINERS.

SEC. 1. *Be it enacted by the General Assembly of the State of North Carolina, and it is hereby enacted by the authority of the same,* That the association of regularly graduated doctors, calling themselves "The State Medical Society," be and they are hereby declared to be a body politic and corporate, to be known and distinguished by the name and style of "The Medical Society of the State of North Carolina," and by that name and style shall have perpetual succession, and a common seal; that they or a majority of them and their successors shall be able and capable of in law to take, demand, receive and possess, money, goods and chattels, land and tenements, and apply the same to the use and for the advancement of the purposes and objects of the said society; that the said Medical Society or a majority of them and their successors shall be able and capable in law of sueing and being sued, pleading and being impleaded; that they shall be authorized to make all by-laws, rules and regulations, necessary and proper for their own government and for carrying out the purposes contemplated in this act, and for the promotion of medical science, and the elevation of the medical profession in this State, not inconsistent with the constitution and laws of North Carolina.

SEC. 2. *Be it further enacted,* That from and after the 15th day of April, 1859, no person shall practice medicine, surgery or any of the branches thereof, or in any case prescribe for the cure of diseases, for fee or reward, unless he or they shall have been first licensed so to do in the manner hereinafter described, *Provided,* that no person who shall practice in violation of this act shall be deemed guilty of a misdemeanor.

SEC. 3. *Be it further enacted*, That in order to the proper regulations of the practice of medicine and surgery in the State of North Carolina there shall be established a board of regularly graduated Physicians to be known by the name and title of "The Board of Medical Examiners of the State of North Carolina.

SEC. 4. *Be it further enacted*, That the Board of Medical Examiners of the State of North Carolina, shall consist of seven regularly graduated Physicians.

SEC. 5. *Be it further enacted*, That it shall be the duty of the said Board to examine all applicants for license to practice medicine and surgery, or any of the branches thereof, in the State of North Carolina, on the following branches of medical science, viz: Anatomy, Physiology, Surgery, Pathology, Medical Hygiene, Chemistry, Pharmacy, Materia Medica, Therapeutics, Obstetrics and the Practice of Medicine, and if on such examination he or they be found competent, to grant to such applicant or applicants a license or diploma authorizing him or them to practice medicine and surgery, or any of the branches thereof, in the State of North Carolina. *Provided*, that five members of the Board shall constitute a quorum, and that four of those present shall be agreed as to the qualifications of the applicant.

SEC. 6. *Be it further enacted*, That the said Board shall be at liberty to examine for and grant license to practice medicine or surgery, or any of the branches thereof, in this State [to or any of the branches thereof in this State] to any person so applying, [who shall give any person so applying,] who shall give satisfactory evidence to the Board that he is 21 years of age, and of good moral character, such applicants if found competent shall have granted to them the license before mentioned, signed by the Board of Medical Examiners or a majority thereof, and if found incompetent they shall be rejected.

SEC. 7. *Be it further enacted*, That to prevent delay and inconvenience two members of the Board of Medical Examiners, may grant a temporary license to applicants therefor, and make report thereof to the next regular meeting of the Board for confirmation, provided that such temporary license shall not continue in force longer than the next regular meeting of the Board, and that such temporary license shall in no case be granted after the applicant has been refused a license by the Board of Medical Examiners.

Sec. 8. *Be it further enacted*, That it shall be the duty of the Medical Society of the State of North Carolina to furnish to the General Assembly of the State of North Carolina by their [Society] Secretary a list of Members of that Society from which list the General Assembly shall elect seven to constitute the Board of Medical Examiners before mentioned, to continue in office for the term of six years from the date of their election. *Provided*, that whenever any member of this Board shall cease to be a Member of "The Medical Society of the State of North Carolina," either by resignation or expulsion, his office of Medical Examiner shall be thereby vacated.

Sec. 9. *Be it further enacted*, That the members of the State Medical Society shall have power to select the Board of Medical Examiners, except when the Legislature chooses to exercise this right.

Sec. 10. *Be it further enacted*, That the Board of Medical Examiners thus appointed shall assemble in the City of Raleigh and Morganton, alternately, on the first Monday in May in every year, and shall remain in session from day to day, until all applicants who may present themselves for examination within the first ten days after their meeting shall have been examined and disposed of.

Sec. 11. *Be it further enacted*, That the Board of Medical Examiners shall be and they are hereby authorized to elect all such officers and to frame all such by-laws as may be necessary to carry this law into effect, and in the event of any vacancy by death, resignation or otherwise, of any member of said Board, the Board, or a quorum thereof, shall be and they are hereby empowered to fill all vacancies.

Sec. 12. *Be it further enacted*, That the Board of Examiners shall keep a regular record of its proceedings in a book kept for that purpose, which shall always be open for inspection, and shall cause to be entered on a book kept for this purpose the names of each applicant for license, and the name of each applicant licensed to practice medicine and surgery, and the time of granting the same, together with the names of the Members of the Board present, and shall publish the names of those licensed in two of the

newspapers published in the City of Raleigh within thirty days after the granting of the same.

SEC. 13. *Be it further enacted*, That the said Board shall have power to demand of each and every applicant thus licensed, the sum of ten dollars before issuing a license or diploma, and the sum of five dollars for each temporary license, to be paid to the Secretary of the Board.

SEC. 14. *Be it further enacted*, That the members of the said Board shall receive as a compensation for their services four dollars each per day during the time of their session in the city of Raleigh, and in addition thereto, their travelling expenses to and from the city of Raleigh, to be paid by the Secretary of the Board, out of any moneys in his hands, upon the certificate of the President of the Board of Examiners.

SEC. 15. *Be it further enacted*, That any person who shall practice medicine or surgery in this State, without having first applied for and obtained license from the said Board of Examiners, as provided for by this act, shall not be entitled to sue for or recover before any magistrate or court in this State, any medical bill for services rendered in the practice of medicine or surgery, or any of the branches thereof.

SEC. 16. *Be it further enacted*, That the said Board shall have the power to rescind any license granted by them, when upon satisfactory proof it shall appear that any Physician thus licensed, has been guilty of grossly immoral conduct.

SEC. 17. *Be it further enacted*, That the Secretary of the Board of Medical Examiners shall give bond with good security to the President of the Board, for the safe keeping and proper payment of all moneys that may come into his hands under the provisions of this act.

SEC. 18. *Be it further enacted*, That the provisions of this act shall not apply to any person or persons now engaged in the practice of medicine or surgery in this State, but shall be construed as applying to those only who may hereafter propose to commence the practice of the same in the State of North Carolina.

SEC. 19. *Be it further enacted*, That this act shall be in force on and after the 15th day of April, 1859, and shall be considered a public act.

Read three times and ratified in General Assembly, this 17th day of February, 1859.

THOMAS SETTLE, JR., S. H. C.
HENRY T. CLARK, S. S.

STATE OF NORTH CAROLINA.
Office of Secretary of State.

I, RUFUS H. PAGE, Secretary of State in and for the State aforesaid, do hereby certify that the foregoing is a true copy of the original on file in this office.

Given under my hand, this 23rd day of February, 1859.

RUFUS H. PAGE,
Secretary of State.

This Bill, though not perfect in all its parts, really accomplishes a great deal for the Profession, and we can but congratulate the friends of Medical Reform upon its passage. We hope to see it put in operation, though we do not exactly understand how this is to be done before the meeting of the next Legislature. Will some of its immediate friends explain?

THE STATE LUNATIC ASYLUM.

UPON the soil of the old North State, and within sight of a Capitol where genius and eloquence have found their proudest illustration, an Institution has been erected, magnificent in architectural design, complete in every arrangement, and devoted to an object that renders it an honor to Carolina and to the age. The schemes of the politician, the triumphs of the partizan, the "fantastic tricks" of the legislator, may absorb the attention of the public for a moment; but there lives not an honest man within the limits of the State, who does not feel a deeper interest in the Lunatic Asylum than in all of these ephemeral objects com-

bined. Patriotism and humanity unite in making appropriate offerings to the spirit of liberality in which this noble work has been executed, and in discovering in its successful operation, abundant cause for thankfulness and exultation. It cannot be questioned that its career has been fruitful of good works. Through its instrumentality the sufferings of the outcast have been alleviated and relieved; the wandering intellect has been allured from its eccentric orbit back to its legitimate sphere; the breaking heart has been bound up by the hands of kindness and skill, and taught to thrill again to the magic touch of love and friendship; and the shattered link of many a sorrowing family circle has been restored in all its pristine strength and brightness, to the eternal gratitude of those who mourned it as lost forever. Through its benign instrumentality the prison house has been entered, and the cankering fetter stricken from the limbs of him whose mental ruin was his only crime. At its bidding the loathsome dens to which affection, in the blindness and infatuation of a false philosophy had confined the objects of its warmest solicitude, gave up their victims to the merciful dispensations of a more judicious philanthropy. Under its gentle and elevating influence a new era has dawned upon the destiny of those unfortunate beings whom Heaven has cursed with insanity, before whose radiant beams the darkness and gloom of many a noble mind and generous heart have been scattered as the mists of the morning by the rising sun, and whose glorious advent has been hailed by the prayers, the praises, and the hosannahs of grateful thousands.

Religion and Science have met in fond embrace within its towering walls, whilst Humanity has dried up the fountain of its tears, and decked in the sunniest of smiles, rejoices over their hallowed union, as the harbinger of a prouder day for the cause of philanthropy and civilization. Dean Swift has well said that

" he who makes two blades of grass grow where one only grew before is a benefactor to the race." But in our judgment, this honorable title is far more deserved by the man who causes the barren desert of a rifled heart " to blossom like the rose;" who plants the germs of gentle, generous and kindly feelings in a bosom desolated by the withering blight of madness, and who pours the blessed sunlight of reason into the cheerless gloom, the dreary vacuity, and midnight darkness of a disordered intellect. This has been the mission of our Asylum; this the exalted privilege of its patriotic founders; and we can but exult over the success of the one and the victories of the other, regarding them as the surest evidences of the improvement and enlightenment of our people, and the noblest trophies with which modern civilization has adorned its triumphal car.

This Institution has but few if any superiors in this country, whether in the elegance and suitability of the buildings, the beauty and salubrity of the situation, the completeness and comfort of the internal arrangements, or the skill and amiability of the superintendent be taken into the account; and every Carolinian should cherish it as an honor to the State. In the members of the Medical Profession, it should especially find zealous and unflinching friends; for the system upon which it is conducted, as well as the policy which proposed and perfected the whole work, is that which has been eliminated and inaugurated by the patient labors of physicians. The kinder and more rational methods of treating insanity, which distinguish the present day, are some of the triumphs of medicine. The rich offerings of love and gratitude with which humanity delights to chronicle the victories of science over prejudice and suffering, should be laid at the feet of Æsculapius. The work is in fact all our own, and as such we should defend it without stint or limitation, alike against the assaults of the ignoramus, the charlatan and the demagogue.

We are pleased to state that the attempt to cripple this noble Institution—which to the disgrace of the late Legislature, was attempted by some of its members—met with the fate it deserved, and was crushed in its birth. God grant that so blind and contracted a policy may never meet with favor at the hands of the people of North Carolina!

DIFFICULTIES IN NEW YORK.

A DISGRACEFUL row has just come off in New York. The Academy of Medicine has been the scene of a regular "set too" between several of its most *distinguished* members. The rivalry between the opposing schools, which has been so long culminating, has at length reached its climax, and blazed forth with an intensity that has startled the whole country. Dr. Horace Green, who, though the most inveterate of *hobby* riders, is really a man of the greatest respectability and the highest standing in his profession, has been publicly arraigned as the author of the death of a certain Mr. Whitney, a patient of his, into whose throat he had inserted a probang in his usual manner. Among his enemies and accusors, we are sorry and surprised to see the name of Dr. Valentine Mott, a man universally regarded as the very Nestor of the profession, and its proudest illustration. For, whether the fatal result was a *post hoc*, or a *propter hoc*, Dr. Green had treated his patient in such a manner as he honestly believed best adapted for the relief of the case, and in accordance with therapeutical principles which his own experience, and the sanction of the highest medical authority in Europe, had established as legitimate and reliable; and it was clearly the duty of the profession, not only to give him the benefit of every possible doubt, but to shield and protect him against public animadversion by all honorable means, and to the last extremity. It seems that in a short time after the application had been made, Whitney became ill, com-

plaining of great difficulty of breathing, pain and swelling about the throat, and harassed by the impression that the doctor had inflicted some serious injury upon him ; and, when in this condition, a Dr. Beales was called upon to attend him. This individual instead of conferring with Dr. Green, and stating the circumstances candidly and fairly, called in Dr. Mott, who, in the most unjust and ungenerous spirit, without proper examination or reflection, immediately adopted the views of the patient, and proclaimed that Whitney was dying from the treatment to which he had been subjected. But this is not all, a *clandestine* post-mortem was eventually held, by a self appointed tribunal, of which Dr. Valentine Mott was chief inquisitor, and a case made out against Dr. Green, though more than a sufficient natual cause for the fatal termination was eliminated by the scalpel.

This secret investigation was a most disreputable thing—disgraceful to those concerned in it, detrimental to the interests of Medicine, and utterly at variance with that honorable courtesy which should distinguish the professional intercourse of all respectable physicians. It was an attempt at an unfair advantage. It was a mean, contemptible and unmanly trick to injure the fair fame of a rival. It was a despicable stab in the dark by the envious hands of those whom the light of day would have palsied with fear or blanched with shame. The deadliest of serpents warns before he strikes, and dies rather than prove a coward in his wrath; but the conduct of these persons was modelled after that of some inferior and more malignant species, in whose stealthy spring and deadly blow, it finds a proper parallel. It is by such inconsiderate and unprofessional conduct as this, that Medicine is brought into disrepute, its votaries dishonored and its noble offices prostituted and debased. It is thus that in despite of every appeal to the honor of physicians, in utter disregard of the ethical codes establised for the regulation of the profession,

and in the most contemptuous defiance both of local and national organizations, the mean enmities, the low jealousies, the envious rivalries of opposing factions, dissipate the utopian dreams of those who love their calling, substitute an era of ill feeling for one of fellowship and fraternity, and induce even the most sanguine of its friends to despair of so hopeless a cause. We are pleased to see that the Academy had the manliness and wisdom to frown down this unjust assault upon one of its members, by declaring that "Whitney came to his death by no improper treatment, but as an unavoidable result of a complication of diseases"—thus rebuking the accusors as they deserved, and sustaining the accused, as the interest and honor of the profession demanded. This case, though so dishonorable in itself, furnishes at least one instructive lesson. It will teach the jealous to be wary in the exhibition of an unjust and illiberal spirit towards their brethren, lest in their intemperate malice such a retribution overtake them as will recoil to their own destruction. It will demonstrate the folly of one man's attempting to build up his own reputation upon the ruin of another's, as well as the imprudence of appealing to the tribunal of public opinion in matters strictly professional. It will show to the world the intimacy and intricacy of the relations which bind all regular practitioners together in one homogeneous body—the strength and delicacy of those ties which unite physicans to each other, and to the whole profession the necessary, absolute and reciprocal dependence sustained by medical men to their brethren both individually and collectively. It will establish conclusively that when one member is attacked, the whole profession is assailed; and that both honor and self-interest unite in demanding from every man in our ranks his countenance and support for a persecuted brother. From this point of view something of advantage *may* accrue to Medicine even from so disreputable a transaction; for, as "the

burnt child dreads the fire," this experience will not be entirely thrown away upon those whose proclivities to the same species of ungenerous rivalry, require a warning and a restraint. This effort to squeeze a little comfort out of this most disreputable transaction, has so completely exhausted us, that we are constrained to abandon the subject and conclude our article.

AN APOLOGY.

We owe an apology to several of our subscribers; not that we have intended the least disrespect or want of courtesy by any means, but simply from a mistake occasioned by a clerical error. Finding that in despite of our urgent appeals, many persons were still in arrears, we concluded to take the trouble to write to them all, requesting an immediate remittance of the amount due from each individual. The services of an assistant was necessarily required in this laborious undertaking, and in the hurry and confusion of directing so large a number of letters, several were addressed to persons who had previously paid their dues. To all who have been thus treated, we make this excuse, offering an humble apology for the mistake, and promising that nothing of the kind shall occur for the future. We can but hope that none will allow themselves to be offended by this thing, as it would pain us greatly for the Journal to lose ground from any act of ours, however innocently committed.

THANKS.

We are under obligations to Messrs. Blanchard & Lea for several valuable works, which shall be duly noticed in some future No. This is one of the first and most liberal publishing houses in America. Their kind attention to the Medical Journal of North Carolina in its infancy, is properly appreciated by ourselves, as it should be by every friend of the enterprize.

THE QUID PRO QUO.

We are continually receiving circulars of schools, inaugural and valedictory addresses, medical pamphlets, and various things of the same character, from localities with which the Journal has no acquaintance, and from persons who are not its patrons. To the schools which have thus honored us, we beg leave to say, that there is an advertising department to this work, in which they can set forth their claims *ad libitum*, provided the published terms are complied with. If they desire special notices, they must apply to such periodicals as they encourage pecuniarily, as these can afford to puff them for the *quid pro quo*. As for the aspiring individuals who wish to use us as the ladder of their ambition, we have only to say, that they must pay *three dollars* for the privilege, for we will not place the crowd of cormorants who infest the journals and live only on their praises, upon the same footing with our regular subscribers. When a man produces a great work, we shall delight in noticing it, and in according to him all merited honor. When a valuable communication is offered to this Journal, we shall thank the author most kindly, and insert it immediately, wherever he resides, or whoever he be. But when an address is delivered, or a paper prepared for the public at large, whether as a means of advertising, or for the purpose of making reputation, and is then sent to us, in common with all the Journals of the country, by some man who has not exhibited sufficient interest in our success to become a subscriber, we shall quickly consign it to our receptacle for rubbish, and leave the enterprising aspirant to whistle for his puff. And, if those who sustain us should attempt a venture in such a field, they may certainly expect a notice at our hands, but whether to their taste or not, must depend entirely upon the merits of the production.

ANNUAL ADDRESSES.

We had intended to begin the publication of these addresses in the present number, but have been unavoidably prevented. Those who have delivered such, will confer a favor by sending copies to us as speedily as possible.

CORRECTIONS.

ERRATA IN DR. HOWARD'S ARTICLE IN THE LAST NUMBER.

1. Page 308, line 25 from top, for *or* stimulated, read *and* stimulated.

2. " 317, " 7 " for spatum read sputum.

3. " 319, " 35 " for *bloody* expectoration, read *fluid* expectoration

4 " 325, " 15 " for *and* icteroid, read *an* icteroid

5. " 329, " 32 " for *mucous* read *mucus*.

6. " 332, at top line, for *pneumonia* read *phenomena*. Also, on page 332, commencing at line 15 from top, insert three words in that sentence, viz.: *form* after "malarial," *the* before remissions, and *an* before active, and *indeed* in place of *instead*, so as to make the sentence read thus:—We observe, in the first place, that according to Dr. Manson's own admission, the febrile movement in the "malarial" form is not always "distinctly paroxysmal and remittent;" indeed, he states positively, that "the remissions are sometimes obscure, and an active fever exists during the whole day and night."

7. Page 333, line 18 from top, for *Rensward* read *Renourd*.

8. " 334, " 24 " erase the words "and do assume the remittent." Also, page 334, line 34 from top, for *process* read *processes*.

PROSPECTUS

OF THE

MEDICAL AND SURGICAL REPORTER.

This Journal, for many years favorably known as a Monthly, was changed to a Weekly on the 1st of October, 1858.

Its aim will be, as heretofore, *to serve the Profession*—especially in its organized capacity—without fear or favor, and entirely untramelled by outside connections, under the following departments :—

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Nothing which our means can command, shall be spared to make the Reporter an able and acceptable exponent of American Medicine and Surgery. To this end we solicit an earnest and hearty literary and pecuniary support.

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S. W. BUTLER, M. D.,
R. J. LEVIS, M. D., } Editors.

NORTH CAROLINA

MEDICAL JOURNAL.

Vol. II. JUNE, 1859. No. 3.

ORIGINAL COMMUNICATIONS AND TRANSLATIONS.

A REVIEW OF DR. OTIS FREDERICK MANSON'S ESSAY ON "MALARIAL PNEUMONIA."

What is "Malarial Pneumonia? Is it a disease so distinct and different from ordinary pneumonia, as to be "unlike in character and nature"—or is it almost always but the ordinary form, complicated by the impression of a malarial law? By W. T. Howard, M.D., of Warrenton, N. C.

Presented to the State Medical Society of N. C., May, 1858.

[Continued from the April Number.]

PART II.

Of the phenomena by which disease may be recognized—*rational symptoms* and *physical signs*—it is admitted at the present time that the latter are the more sure and certain, and consequently less likely to mislead those duly skilled in their interpretation. Both are of great value, and they should be studied in connection. Nor should either be cultivated to the neglect of the other. But as physical signs depend upon physical changes of structure, and are the indices of those changes, they are due to causes less complicated and more constant in their nature than are rational symptoms. At the same time, physical signs are less subject to variation, and from the very source of their origin directly challenge the cognizance of the senses; and when they do vary it is to keep pace with the progress of disease and very generally, if not always, accurately to define its limits.

Rational symptoms, on the other hand, being dependent upon the vital property, sensibility, are often greatly influenced by the state of the nervous system. Thus, inflammation of an important organ may exist, and induce frightful destruction of its tissues, without eliciting any noticeable amount of pain; but in diseases of the chest more especially, this cannot occur without producing physical signs of those altered physical conditions, and fortunately, these are almost always available for diagnosis. Hence, as Walshe aptly remarks, physical signs may be regarded as instruments of pursuing morbid anatomy on the living body. It follows therefore that all who desire to excel in the difficult art of exact diagnosis, and to trace home to their true causes the diversified mutations of morbid actions, as they are often but dimly reflected in the modified vital properties of diseased organs, should assiduously devote themselves to the careful study of physical signs, and watch with jealous vigilance all efforts, whether they be openly avowed or undesignedly made, that may have a tendency to undermine their importance or lead to their neglect.

We have read with care Dr. Manson's account of the physical signs of "malarial pneumonia," and are constrained to say that, if the opinions he has expressed, and the experience he has disclosed, shall be verified by those who are competent to form correct conclusions in the premises, then, the pathetic exclamation of Baglivi—

> O quantum difficile est curare morbus pulmonum!
> *O quanto difficilius eosdem cognoscere!*

which Dr. Manson prefixes as a suitable motto for his essay, is, indeed, one that is exceedingly appropriate for a dissertation on a disease of the lungs, even at this day, notwithstanding the vast flood of light which has since been shed upon the subject. We had though however with Balfour, that the mournful wail of Baglivi has been answered by the glad Eureka of Lænnec.

We shall now proceed to examine the grounds upon which Dr. Manson bases his views, and to contest their general accuracy and validity. That he may be fairly and fully represented, we

shall give his presentation of the subject *in extenso*, "word for word and comma for comma"—beginning with his description of the physical signs of the first stage of the disease.

"Physical Signs and Physical Changes—First stage, engorgement or active congestion.

"Advancing no claim to expertness in physical diagnosis, I feel a great hesitation in expressing my opinion in regard to the import and value of morbid sounds in this affection. I feel it my duty, however, to say that the crepitous rhonchus, regarded by Lænnec and his imitators, as the earliest pathognomonic sign of pneumonic inflammation in general, is not usually present at the commencement, or, indeed, any of the stages of this disease. That it is sometimes heard, we know, but that it is so commonly observed as to possess alone any very important signification in estimating the extent, precise seat or gravity of the disease, I do not with my present experience believe. Its pathognomonic value called into question at the time of its promulgation, by Andral,* Chomel,† Cruvelhier and others, has been more recently and strenuously contested by Stokes ¶ and the celebrated Professor Skoda, ‡ of the school of Vienna—I therefore feel more confidence in candidly expressing my views. It is difficult, after perusing the works of Lænnec, Williams, Walshe, Barth and Roger, and others, and observing the positive declarations made by them in regard to the existence and value of this sound, to withhold a belief in their statements. On the other hand, it is equally impossible to consult the distinguished authorities above cited, without being convinced that they are penned by skillful and truthful observers. I do not know how these conflicting views can be reconciled, save by the supposition that their authors were observing different forms of pneumonic lesion. ‖

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* Cl. Med. tome ii., 333.

† Dict. de Med tome xvii., 232.

¶ Diseases of the Chest.

‡ Aus. and Percus, Markham's translations, 151, 157, 303, 306, 315, 323.

‖ Laennec's descriptions are evidently taken from cases of simple sthenic pneumonia.

"If the peculiar variety of pneumonia now under consideration, or if this phrase is objectionable, if the condition of the organism with which inflammation of the substance of the lungs in the present disease is found associated, is widely different from that described by the majority of authors, then it is reasonable to suppose that some diversity in the physical signs would be observed. If, in this disease, as we shall contend, the pathological condition of the lung usually partakes more of the character of congestion, than open inflammatory action, then the absence of crepitation may be explained, without impeaching the testimony of those who have sought to affix it to the first stage of pneumonia as the diagnostic sound. The presence of crepitating rale is justly and reasonably ascribed to the passage of air through the viscid pathognomonic secretion in the air cells and minute bronchi; that secretion being considered the result of inflammation. Now, although this fluid, as proven by its expectoration, is present in all varieties of pneumonia, yet it is usually more fluid, and more sanguineous in this than in simple pneumonia, and we should, therefore, rationally conclude that the passage of air through fluids of varying tenacity, density and composition, would produce sounds of a variable character.* Again, it often occurs in the early stages of this affection that the cough is unattended with expectoration, for several days; indeed, we have seen cases of the most violent character, terminating in death by apnœa, with expectoration scarcely sufficient in quantity to betray the character of the affection; the cough being dry throughout, and the dyspnœa extreme. Now, in these cases, we should be prepared to learn that no crepitation would be heard, or, if observed, not in proportion to the extent or gravity of the pulmonary lesion. Again, in describing the anatomical characters of pneumonia, Lænnec† admits that inflammation of the bronchia is a frequent attendant on the simple form, "its lining membrane being *commonly* very red," "occasionally swollen," "the redness sometimes extending over the whole bronchi." Now, if this be

*Lænnec himself only cites two cases of this form of pneumonia, in which the glutinous sputa and crepitous rhonchus were observed.—(Forbes' translation, 249.)

†Forbes' translation, 220.

true, and who will gainsay it, how is it possible that sonorous, sibilous and mucous rhonchi should not be often heard, as maintained by Skoda,* and if present, drowning, according to Andral,† the less audible sound of crepitation. But, as has been premised, as the anatomical character of this affection is doubtless an intense congestion of the parenchyma, associated, perhaps, in some degree, with inflammation, it is probable that the absence of crepitation in the large majority of cases may be thus explained.

"In the first stage of this disease, we may have, therefore, the crepitating rale, or mucous rales, in varying degrees, accordingly as the character of the case may partake of inflammation or active engorgement. In many cases, however, the practitioner will fail to observe any morbid sound, although the concurrent symptoms may render the diagnosis positive. In these instances, the crepitating rale may sometimes be elicited by engaging the patient to cough forcibly, as advised by Barth and Roger.‡ Although, as I believe, crepitation is neither pathognominic, at least of this variety of pneumonia, or confined to inflammatory affections of the parenchyma, being simply the sound produced by the passage of air through a viscid fluid in minute tubes; yet when present, its importance should not be undervalued, as when taken in connection with other signs and symptoms, it affords a valuable aid to diagnosis. There are however, as has been already indicated, other sounds observed in the first stage of this disease. Early in the invasion, in some cases on the first day of its existence, there is a copious sero-sanguineous secretion or exhalation present, obstructing the current of air in the bronchiæ, trachea and larynx. In these instances, we have, therefore, abundant bronchial, tracheal and laryngeal rales, which may be often heard at some distance from the patient. On the other hand, as we have observed, all sounds indicative of disease are

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*306.

†Note to Lænnec.—(Forbes' translation, 246.) It is traditionally well known in Paris, that even Lænnec committed the error of confounding double capillary bronchitis with double pneumonia.—Walshe, Amer. ed., 366.

‡Prac. Treatise, Aus. Edinburg ed., 141.

absent. Dyspnœa, pain, cough, peculiar sputa and every other symptom may be present, yet no sound save the vesicular murmur, perhaps louder than in health, is heard, the chest being at the same time clearly resonant on percussion. In these cases the pneumonia is either central, or the secretion in the air-cells and finer branches of the bronchi may not be present in quantity sufficient to offer any obstacle to the transmission of air. Whatever explanation however, may be offered, I am fully persuaded, after careful exploration of the chest in many cases daily and hourly made, that at least in the early stage of this form of pneumonia, no abnormal sounds can often be observed."

Such is Dr. Manson's description of the physical signs of the first stage of "malarial pneumonia." It will be perceived that he "advances no claim to *expertness* in physical diagnosis." We would not assert for ourselves any extraordinary proficiency in the art. Nevertheless, he who ventures to give an account of the physical signs of pneumonia, or of any other thoracic affection, certainly does assume, in the very act of so doing, that he is at least so far competent to the task as to entitle his views to the respectful consideration of the profession, or else it is quite obvious, that his opinions are without value. And when he attempts to sustain opinions differing very widely from those generally entertained by the ablest writers upon Physical Diagnosis, by references to other eminent authorities, and thus to fortify himself with more "confidence in candidly expressing his views," it is alike natural and proper that we should closely scrutinize the foundations upon which those opinions rest, and rigidly compare them with those of the authors he cites as confirming his own.

In the first place, we cannot forbear an avowal of surprise that Dr. Manson should seek to give weight to his views by confirmatory references *to* "*British and Continental writers;*" for he had boldly declared in the beginning of his Essay that, "however perfect in all points for *those regions*, they are *sadly deficient for us;*" and that the peculiar form of pneumonia he describes, "has not attracted the attention of recent investigators, and is only cursorily and imperfectly noticed by any of the modern standard authorities!" Surely then, it is not a little remarkable

that no other authorities whatever should be alluded to in this production; that Dr. Manson has not honored his own countrymen even with the complimentary tribute of a passing reference; and that only in the works of "*British and Continental writers*," which are represented by him to be so "*sadly deficient for us*;" has he been fortunate enough to find ample corroborative testimony, as he imagines, when he is desirous of supporting his disparaging estimate of the *crepitant rhonchus* by the weight of eminent names! Now, it follows of necessity, either that the authors referred to by Dr. Manson, had in view the same form of pneumonia that he has described—and if so, what becomes of the broad statement that "unfortunately for the student of the present day," it has been so "sadly" neglected by them?—or, that after all—notwithstanding what he has said—he has illogically mustered authorities on his side of the question, which, according to his own testimony, are inapplicable.

Nor will it escape notice that Dr. Manson speaks of the many celebrated men who have verified the teachings of the illustrious Lænnec in regard to the great importance of the crepitant rhonchus as the earliest pathognomonic sign of pneumonia, as "his *imitators!*" There are many excellent observers who have adopted Lænnec's views in respect to the frequency of occurrence, and diagnostic value of this râle in pneumonia, who have differed from him on other points; and their labors have materially enlarged the boundaries of the Science of Auscultation. They deserve, it seems to us, a higher position in the Temple of Science than that so unceremoniously assigned them in the somewhat obscure corner of Imitation. But if there is anything in the charge, Dr. Manson has himself, if we mistake not, imitated "the celebrated Professor Skoda of the School of Vienna," and has in a striking manner committed the error charged upon his brethren in the South, of "forcing the forms" of at least one of "our diseases to bend to the descriptions" of the Viennese Professor; and has also "forced" the views of this Professor, as well as those *of others*, to sustain his own opinions.

I. Reverting to the first paragraph in the above long quotation, let us endeavor to analyze Dr. Manson's views in regard to

the *crepitant rhonchus*, and to determine what are the the doctrines he seeks to establish. It seems to us that in this paragraph Dr. Manson either distinctly states, or necessarily implies the truth of the following propositions:

1. That the *crepitous rhonchus*, regarded by Lænnec and his imitators as the earliest pathognomonic sign of pneumonia, is not usually present at the commencement, or, indeed, any of the stages of "*malarial pneumonia.*"

2. That though sometimes heard, yet it is not so commonly observed as to possess alone any very important signification in estimating the extent, precise seat or gravity of the disease.

3. That its pathognomic value was questioned at the time of its promulgation, by Andral, Chomel, Cruvelhier, and others, and has been more recently and strenuously contested by Stokes and Skoda.

4. That the views of these writers, in regard to the value of this *rale*, differ so widely from those of Lænnec, Williams, Walshe, Barth and Roger, that they cannot be reconciled with them, save by the supposition that their authors were observing *different forms of pneumonic lesion.*

5. That, inferentially, as Andral, Chomel, Cruvelhier, Stokes, and Skoda, in their estimate of this räle, have imparted confidence to him in the opinions he entertains respecting its value, *he* and *they* observed the *same form* of the disease.

We now propose to inquire how far these statements comport with the *facts* in the case.

It will not be proper to discuss them in the order in which they are here presented; for, as physical signs depend upon altered physical conditions of the lungs, it becomes necessary, in the first place, to ascertain *what was the "form of pneumonic lesion,"*—what are the *anatomical characters* of the disease—these eminent men observed? If we can prove that their descriptions have reference to the *same form of pneumonia that Lænnec described*, then the very foundation upon which Dr. Manson seeks to discover and to reconcile a supposed difference on their part respecting the value of the *crepitant rhonchus* as a physical sign of the first stage of the disease, will be swept away. Fortunately, this task

is easily accomplished, for, unlike Dr. Manson, they (so far as our knowledge of their writings extends,) have left us in no doubt respecting the "forms of *pneumonic lesion*" that they "observed."

In the first place, is it not wonderful that Dr. Manson should have resorted to vague "SUPPOSITION" in respect to what are the facts, when the facts themselves were before his eyes? He refers to the above named writers as familiar authorities. He speaks of them on the one side, as making statements in regard to which it is "difficult to withhold a belief"; and on the other as "skillful and truthful observers"; and by all the indications of ready reference and facile allusion, he would betoken an easy and off-hand familiarity with their writings. Such, at least, is the impression made by this essay. Why, then, could he not determine from their "recorded" observations what was the form of pneumonia that they described? He states in a note that "*Lænnec's* descriptions are *evidently* taken from cases of *simple sthenic pneumonia*", as if there were some doubt respecting the descriptions of the other authorities named! Now, it seems to us to be impossible for any one to read the articles on pneumonia by such of the above authors as have written essays on the subject, and not see that they had in view the very same form of pneumonia that *Lænnec* described. This, indeed, is what they profess to have done. Will it be said that such observers did not know what they were about? Will that bear telling? It is very true that, in regard to the exact pathology of the disease—as, for instance, what constitutes *hepatization* in the second stage, whether it be caused by lymph-exudation or a mere congestion of the blood—some of these authors differ. Still it is the very same *form* of pneumonia that *Lænnec* described that *they* describe, as is proven by their descriptions of the vital phenomena, physical signs, and pathological anatomy. All this is so patent and obvious from their writings, that Dr. Manson must, indeed, have resorted to mere "supposition" to have arrived at a different conclusion. In order to substantiate this statement, it cannot be expected that we should insert their descriptions of the anatomical characters of all the several stages of ordinary acute lobar* pneumonitis,

*Dr. Manson's own descriptions show that he is treating of *lobar* pneumonia. He contends, however, as we shall see hereafter, that there are two varieties of this form. All other forms of pneumonia,—i. e. *lobular* pneumonia, *vesicular* pneumonia, (the *vesicular*

into which the disease was first divided and described by Lænnec. That would occupy entirely too much space. Nor is it necessary for the object now in view. At this time we are investigating only the *first* stage; and it is for Dr. Manson *to show from their descriptions of the pathological anatomy of this stage*, that they differ so widely, and in such a manner as to account for his assumed difference on their part "in regard to the existence and value of the *crepitant rhonchus* as a physical sign indicative of it. That the reader may the more readily compare the descriptions of the first stage, by those that are supposed by *Dr. Manson* to have "observed different forms of *pneumonic lesion*," we shall arrange some of them side by side in parallel columns.

Anatomical Characters of the first stage (engorgement) of Pneumonia.

1

Laennec.

"In this degree the lung is externally of a livid or violet hue, heavier and much more solid than natural. It is however still crepitous, but much less so than in a sound state, and on pressing it between the fingers, we perceive that it is injected by a liquid. It retains the impression of the fingers nearly like an œdematous limb. When cut into, it appears of a livid or blood color; is quite injected with a frothy serous fluid, more or less sanguineous, which flows from it abundantly. We can still however discover very clearly, the natural alveolar and spungy texture of the viscus, except in some points, where the obstruction is more solid and compact, indicating the transition from the first to the second degree of pneumonia." —Diseases of the Chest, American Ed., (1838,) 209.

Chomel.

"In this first degree of inflammation, the lung, exteriorly, is of a more or less deep red colour, often livid and sometimes blackish (noirâtre); it is heavier than natural, but does not sink in water; it is less elastic and less crepitant than in the healthy state. In pressing it we perceive that it contains a liquid, and on further examination it may be observed that it preserves the impression made upon it by the fingers, as though infiltrated. When cut into, the cut surface is found to be red like the exterior, and there runs from it abundantly a bloody serum mixed with a certain quantity of air. The parenchyma, having become a little more compact, has lost, however, its cohesion, and allows itself to be penetrated by the fingers, and is more easily torn than in the natural state. This diminution of cohesive power is an important character, but not always sufficient to distinguish inflammatory engorgement from that which takes place during the last moments of life. In examining attentively the pulmonary tissue, we still recognize its alveolar and spongy nature, but the cells are partly obstructed, and their divisions are more or less injected." —Dict. de Med. T. xxv., 146—7, Paris, 1842.

bronchitis of Rilliet and Barthez); the *interstitial* pneumonia of Rokitansky, (see Man Patho. Anatomy, by Carl Rokitansky, M. D., Vol. IV. 78,) &c.—are, of course, out of the question in the discussion now in hand.

2

C. J. B. Williams.

"In the first degree of inflammation, the engorged or obstructed state, the lung is externally of a dark or livid red color, to which a slight, whitish opacity of the pleura sometimes gives a violet hue; it is heavier and more substantial to the touch than a healthy lung; when pressed it yields less of the crepitating sound and feel, and instead of generally collapsing under the pressure, and partially rising on its removal, it receives and retains the impression of each finger, giving the feeling of a liquid contained in cellular substance, as in an œdematous limb. On examining it closely through the transparent pleura, there are seen numerous little round bubbles of air, without the angularity of the naturalvesicular structure;and the septa, which are commonly very visible, are scarcely to be discerned. On cutting into the lung in this state, a bloody serum containing numerous minute air-bubbles flows from it; sometimes in great abundance, and almost clear; sometimes more scanty and sanguinolent, or turbid, and with various proportions of the spumous bubbles. The substance thus cut into is of a red color of various shades, crimson, dark red, brown red, chocolate red, or of a livid puce color, approaching to black The vesicular texture of the lung can still be discerned, particularly after the serum has drained from it; and by pressing it gently and washing it repeatedly, the natural colour, appearance, and elasticity may in some degree be restored.** The progress which the inflammation has made is pretty accurately represented by the defect of air inthe tissue; in the slightest degree,there being a good deal of air, the lung feels still crepitous, and the serum which flows from it is very frothy.** Although very much heavier than natural, a portion of lung in this state retains enough air to prevent it from sinking in water. Another remarkable change wrought in the lung by the most considerable degrees of inflammatory engorgement, is a diminution in its molecular cohesion, so that the fingers break through or tear its substance much more readily than in a healthy. lung."—Cyc. Prac. Med., Vol.III., 594—5.

Andral.

"In its minor degree of inflammation, in that degree designated by the name of simple engorgement, the pulmonary parenchyma still crepitates; it may be pressed and drawn with considerable force without being torn. However, the crepitation is less than in the healthy state. On compressing the tissue of the organ, it is perceived that there is more liquid than air in the pulmonary vesicles. Often times this crepitation then becomes so little, that one would suppose he pressed the lung of a fœtus which had not respired. The *color* of the engorged part is changed; its brown or vermillion color contrasting with the grey or pale rose-colored tint of the portions of the lung which have remained healthy. If an incision be made into these engorged parts, a reddish frothy serum is seen to flow from them in considerable quantity. If the engorged parts, when cut into in different directions, be subjected to a long-continued pressure, and to frequent washing. these parts are freed from the liquid which they contained, and they are rendered as crepitating, as elastic and as void of color, as the portions of the lung not affected with inflammation. This simple engorgement, without any other alteration of texture, is observed only in the case of slight inflammation. But however little the intensity of the inflammation may be, the pulmonary parenchyma diminishes in consistence; when pressed between the fingers, it crumbles with ease; it becomes friable. The liquid which flows from it, is less copious and particularly less frothy; it then presents considerable resemblance to the tissue of certain spleens, which are easily crumbled, without, however, being reduced to a pulp. This state constitutes an intermediate state between simple engorgement and the real red softening (the hepatization of authors.) It is the transition from the first to the second degree of pleuro-pneumonia."—Med. Clinic, Phild Edition, 1843, Vol. ii., 175—6.

3

Walshe.

"In the stage of engorgement, the external surface of the affected parts is of livid or violet colour, or mottled with both tints and with dull red; the weight of the organ is increased, its elasticity diminished; it retains marks of pressure like an œdematous limb, and crepitates imperfectly. On section reddish spumous serosity escapes in greater or less quantity; the spongy character of lung is still to be recognised; the consistence is diminished. The permeability to air is lessened, not destroyed; the tissue floats in water, even after forcible pressure with the fingers."—Diseases of the Lungs, &c., London Ed (1854.) 408-9.

Stokes.

This author does not describe the anatomical characters of the several stages of pneumonia very minutely. But the following quotation shows beyond all doubt that he had reference to the same form of "pneumonic lesion" that Lænnec described.

"Lænnec has described three stages of pneumonia, with their corresponding symptoms and signs, and *his statements have been verified by all subsequent experience.* In his first stage, the lung is engorged with blood, and a *crepitating rale* is heard; in the second solidity takes place, with its accompanying physical signs; and in the third we find the interstitial suppuration of the lung, or the condition which precedes the formation of abscess. *Without impugning the accuracy of this description,* we may enquire whether a stage previous to that which Lænnec calls the first does not exist. * * * I have repeatedly seen a condition of the lung which seems really the first stage. The pulmonary tissue is drier than natural, not at all engorged, as in Lænnec's first stage, and of a bright vermillion color, from intense arterial injection."*—Diseases of the Chest, 266.

4. We regret not being able to give Cruvelhier's description of the anatomical characters of pneumonia in the first degree. Dr. Manson refers to no work by this writer as his authority for the opinions attributed to him. We have endeavored to obtain a copy of his Pathological Anatomy, but without success. Fortunately however, Swett, in confessing his inability to point out

* Dr. J. Da Costa—in the ablest paper on the minute pathological anatomy of acute pneumonia that has appeared in this, or perhaps any other country—uses the following language in reference to Dr. Stokes' first stage of acute primary pneumonia: "Dr. Stokes' describes as the first true stage, an 'irritation' of the lung in which the pulmonary tissue is drier, and of a bright vermilion colour. However convenient, in many respects, it might be to admit such a morbid condition, pathological demonstrations have not borne out this author's view. Nearly all eminent pathological anatomists of the day deny this stage as an antecedent of inflamation of the lung; nor does, indeed, Dr. Stokes himself seem to have based his assertion so much on anatomical as on clinical grounds. He states, it is true, that he has met with this condition in pathological specimens, and relates a case of an extensive burn in a child, where nearly the whole of the lung was in this state; but he fails to point out that this appearance is of necessity connected with inflamation. I have had recently an opportunity of examining the lung in a case similar to the one he describes. The organ in this instance, was dry, and presented an unusually bright colour; yet nothing could be discovered in which it differed from ordinary congested lungs, nor did it, when minutely examined, afford any evidence of inflammatory action."—*Amer. Jour. Med. Sci., N. S.*, Vol. xxx., 300.

diagnostic differences between a simple stasis of the blood and inflammatory congestion, takes occasion to make a statement in regard to Cruevelhier's views of the matter, which enables us, perhaps, to supply the deficiency. It is well known that the postero-inferior portions of the lungs, and particularly of those in whom the agony was protracted, present appearances that resemble the first stages of pneumonia very closely, and that they are with great difficulty distinguished, if, indeed, it be possible by their physical characters alone. Swett says that "Cruevelhier has explained the difficulty, by maintaining that the two conditions are *identical in nature as well as appearance*—that they are, in fact, both owing to *inflammatory* congestion—the one, occurring during the last moments of life, and usually limited to the most dependent portion of the lungs; the other, occurring at an earlier period, and affecting every portion of the pulmonary substance, and almost always associated with the second stage of the disease,"(*a*). Now, if the reader will revert to Dr. Manson's description of the Physical Signs of the first stage of "Malarial pneumonia," he will perceive that Dr. Manson endeavors to account for his statement of the alleged absence of the crepitant rhonchus in the large majority of cases, by assuming that in the first stage of the disease, "the pathological condition of the lung usually partakes more of the character of *congestion*, than open *inflammatory* action." But if Cruevelhier considers even hypostatic congestion of the lungs to be inflammatory in its character—to be identical with the first stage of true pneumonia, as well in nature as in appearance—it is not likely that he had reference in speaking of the crepitant rhonchus, to a "form of *pneumonic* lesion," in which the congestion was not *inflammatory*.

5. We come now to Skoda—evidently our author's especial favorite. It is certainly reasonable to suppose that Dr. Manson would be accurately informed in regard to his opinions. But we shall soon see that, in respect to the point now at issue, it is impossible that Dr. Manson could be more mistaken about any thing whatever. As Skoda is simply a work on Auscultation and Percussion, it did not fall within the scope of his purpose to

(*a*) Op. Cit., 67.

describe the *pathological anatomy* of the diseases, of which he gives, the *physical signs.* But in commenting upon the auscultatory signs of pneumonia, as described by Lænnec, he subjoins the following note, which places the matter in question beyond all controversy :

"Lænnec seems to have very frequently observed the crepitating *rale* ; I rarely meet with it. The reason of this difference has been accounted for, by supposing that when Lænnec made his observations, pneumonia was of a different character to what it is now, being at present of a gastric type, and not pure. The division of pneumonia into pure, gastric, bilious, etc.,—although of ancient date, *is altogether based upon theory.* As regards the *anatomical conditions of the lungs in pneumonia,* they seem to be at the present time EXACTLY SUCH AS DESCRIBED BY LÆNNEC."(*b.*)

Comment upon this language is surely unnecessary, is it not really surprising that Dr. Manson should have referred the reader to the *very page* in Skoda's work from which the above quotation is made, as indicating this author's views of the crepitant rhonchus, and which needs only to be read to refute Dr. Manson's "*supposition*"? Dr. Manson seems to have been very forcibly struck with Skoda's statement, that he rarely meets with the *crepitant rhonchus* in pneumonia as described by Lænnec ; but entirely overlooks Skoda's positive declaration that the *pathological anatomy* of the pneumonia to which he has reference is "EXACTLY SUCH AS DESCRIBED BY LÆNNEC"—a declaration, too, that was introduced on purpose to exclude the very conclusion that Dr. Manson has drawn! Indeed, Skoda goes much further than this, and denies even the very existence of *bilious,* or "*malarial,*" pneumonia—affirming that that division of pneumonia "IS ALTOGETHER BASED UPON THEORY." It follows, then, that even in regard to the views of his favorite author, Dr. Manson has hazarded a "supposition" not only without evidence to support it, but against the positive and emphatic testimony of that author himself! This looks to us not very unlike "forcing" the opinions of Skoda to "bend" to Dr. Manson's "suppositions."

(*b*) Aus. and Percus. by Joseph Skoda, Markham's Trans., 315.

6. Lastly under this head, we reach M. M. Barth, and Roger. These authors have nothing to say of the *pathological anatomy* of pneumonia in their excellent treatise on Auscultation to which Dr. Manson refers; but as Dr. Manson *admits* (and well he might) that their descriptions of the physical signs of pneumonia have reference to the same form of the disease that *Lænnec* described, this is not a position for us to establish. We shall show, however, at the proper place, that Dr. Manson has strangely mistaken their views in regard to the crepitant rhonchus being considered by them as *absolutely phathognomonic* of *pneumonia.*

We have now looked at all the authors mentioned by Dr. Manson, whose views are accessible, and what have we seen? Is it possible within the entire domain of pathology, to find a more perfect unanimity of opinion in regard to any one point, than they exhibit in relation to the anatomical characteristics of the first stage of pneumonia? It is very true, as already premised, that some of these authors differ in respect to what constitutes the precise pathology of the disease; but we cannot help thinking that Dr. Manson stands alone even in the "*supposition*" that they drew their descriptions from a form of pneumonia different from that described by *Lænnec;* and were it not that so respectable a physician as Dr. Manson has expressed a contrary opinion, we should feel under the necessity of apologizing to the reader for having dwelt so long upon a matter that is so perfectly obvious. Indeed, if the very same lung had been presented to Lænnec, Andral, Chomel, Williams and Walshe, or to any other five competent pathologists—is it probable that it would have been described in language more nearly alike?

III. Having ascertained what was the "form of *pneumonic lesion*" the above authors "observed," and that on this score there are no "conflicting views" to "be reconciled," we are naturally led to inquire what value they attach to the *crepitant rhonchus* as a physical sign of its first stage. We have seen that Dr. Manson places a very low estimate upon this *rale;* that he regards it as usually absent throughout the career of "*malarial* pneumonia" at least; and even when present, that it does not of itself possess any very important significance in denoting the extent or precise

seat of the disease. In confirmation of his views, he urges that its *pathognomonic* value has been questioned by Andral, Chomel, Cruvelhier, Stokes and Skoda, and immediately adds: "*I therefore feel more confidence in candidly expressing my views.*" These "distinguished authorities" are thus cited and arranged in such a manner as to produce on the mind of the reader the natural, obvious, and only rational impression that they agree with him in—or at least give countenance to—his very disparaging opinion of the *rale*; and, of course, that they differ very widely from *Lænnec* in his high estimate of its importance.

Now, had Dr. Manson contented himself with the simple enunciation that the crepitant rhonchus is not *constantly* observed in *pneumonia*, and in the strict, literal, etymological sense of the word, is not *pathognomonic* of—always indicative of and *peculiar* to—*pneumonia*, no body at this day would have questioned the correctness of the statement. But Dr. Manson's language means far more than this, or it has no meaning at all. In stating that the crepitant rhonchus is not usually present either at the commencement, or any other stage of pneumonia; that it is sometimes heard, but that it professes no very important signification in estimating the extent or precise seat of the disease, he goes far beyond a mere critical distinction in the use of terms, and broadly alleges the usual absence of the *rale* in this affection, and its immaterial diagnostic value when it is present; and by an argumentative antithesis of arrangement, as well as by direct and palpable inference, he states as strongly as if by express averment, that the authors whom he has so nicely balanced in "conflicting" array against the chevaliers of a different creed, impart to him encouragement and confirmation *by the coincidence of their views with his own.* If this be not the true interpretation of his language, it is quite impossible to see why or wherefore he should have been inspired by them with "*more confidence* in candidly expressing his views." Surely, then, we should expect to find that Andral, Chomel, Cruvelhier, Stokes and Skoda—by whom his greater confidence was inspired—substantially agree with Dr. Manson in his disparaging opinion of this *rale*; and surely we should also expect to find that Lænnec, Williams, Walshe, Barth and Roger substantially differed in opinion from

those authors on that point. In regard to Cruvelhier, more anon ; but what will the reader say when it is shown that Lænnec, Andral, Chomel, Stokes, Williams, Walshe, Barth and Roger, all substantially concur in the same high estimate of this *rale*, and that Skoda alone is a *quasi* adherent of a different opinion. And yet such are the facts, as we shall now proceed to show.

1. As Lænnec was the great pioneer in Physical Diagnosis, and the first to describe the crepitant rhonchus, let us first determine what he taught in regard to it. We shall quote from the first edition of his work—published within three years of the time when his attention was first directed to the subject of Auscultation—that his views from the very beginning may be seen.

Lænnec's opinion of the *crepitous rhonchus :*

" In the first degree of peripneumony, the respiratory murmur is still heard in the part affected, whether percussion affords any sensible alteration of sound or not ; and it is, further, accompanied by that species of *rattle* to which I give the name of *crepitous rattle*, and which is the pathognomonic sign of this first degree of peripneumony. This species of rattle resembles the crepitation of solid salts in a heated vessel, or it may be said to be very analogous to the noise emitted by the healthy lungs when compressed in the hand—only stronger. The only *other diseases* in which this species of rattle is found, are œdema of *the lungs* and *hæmoptysis*."

Again—in treating of *œdema* he says there is :

" A slight crepitous rattle like that in the first degree of peripneumony, but fainter. Indeed, the *cylinder is not sufficient of itself, without reference to the general symptoms, to distinguish the last mentioned disease from œdema.*" (*a.*)

In the last edition of his work on Diseases of the Chest, after describing more fully the physical signs of pneumonia in the first degree, he adds :

" Of these the most important is unquestionably the crepitous rhonchus ; inasmuch as it is invariably present, and from the very invasion of the disease ; and exists in no other case, *except* in *œdema of the lungs* and *pulmonary apoplexy*,(*b*) two diseases which are easily distinguished from this by their own peculiar signs and symptoms. M. Andral is mistaken in saying that the crepitous rhonchus sometimes exists in simple acute bronchitis ; (Cl. Med. tome. ii. p. 333 ;) and I think this is evident from his own cases. From its constant presence in this disease, I regard it as the most practically useful of all the

(*a*) A Treat. on Dis. of the Chest, &c., by means of Acoustick Instruments, translated from the French of R. T. H. Lænnec, M.D., by Jno. Forbes, M. D. First American Ed. (Philadelphia, 1823). 228, 238, 239.

(*b*) Hæmoptysis and Pulmonary Apoplexy, were used synonymously by Lænnec in his first Ed. In the last Ed., he applies the former term to slight or moderate cases of (according to him,) simple exhalation of blood from the bronchial membrane—reserving the term *Pulmonary Apoplexy* for severe cases originating chiefly in the vesicular structure of the lungs.

stethoscopic signs, inasmuch as it points out, in its very earliest stage, one of the most severe and most common diseases, and thereby enables the physician to apply his means with much more chance of success than he could have done even a few hours later."(*a*.)

Now, if we examine somewhat closely the language used by Lænnec in the first edition of his treatise, it does not appear to be by any means certain that, by stating that the crepitous rhonchus is *pathognomonic* of the *first degree* of peripneumony, he meant broadly to inculcate the doctrine that it is a sign absolutely *peculiar* to—and not to be heard in any other disease than—*pneumonia*; for he tells us, almost in the next breath, that this species of rattle is found in *two other diseases*; and that in one of them—*œdema* of the lungs—the *stethoscopic characters* of the *rale* alone are not sufficient, without reference to the general symptoms, to determine its pathological significance. And even in the last edition of his work, (while stating in his description of the physical signs of the disease,) that "the crepitous rhonchus is the pathognomonic sign of the first stage of peripneumony," he takes care to add on the next page—it "exists in no *other* case, EXCEPT in *œdema* of the lungs and *pulmonary apoplexy*, two diseases which are easily distinguished from this, by their peculiar signs and symptoms." In a preceding chapter, (devoted to the description of auscultatory signs in general,) he had stated that in *œdema* of the lungs and sometimes in *pulmonary apoplexy*, the bubbles "USUALLY seem to be somewhat larger and moister than in the rhonchus of peripneumony"—forming, he says, the *sub-crepitous* rhonchus; and in this, all subsequent experience bears him out in the immense majority of cases; still, in the chapter on *œdema* he affirms, (in reference to the diagnosis between it and pneumonia,) "it must be admitted that it is sometimes difficult to distinguish these two diseases by the *stethoscope alone*, without taking into account the general symptoms;" and in the chapter on *pulmonary apoplexy* (referring to its hæmoptysical engorgement and pneumonia,) he states "that in *both* these diseases, there exists the *same* crepitous rhonchus" (*b*).

(*a*) A Treat. on Dis. of the Chest. Forbes' Trans., with Andral's Notes, (1838), 221.

(*b*) Op. Cit., last ed., 56, 189, 203, 221, 222.

Notwithstanding, then, the customary construction of Lænnec's teaching on this head, it is not as it seems to us, a matter of absolute certainty that he considered the crepitant rhonchus as *strictly pathognomonic* of—as always confined, peculiar to, and not to be heard in any other affection than—*pneumonia.* Would it not be more correct to interpret him thus: "As the crepitous rhonchus is invariably present in *pneumonia*—is perceptible from the very invasion of the inflammation—and exists in no other case, except in *œdema* of the lungs and *pulmonary apoplexy*, two diseases which are easily distinguished from this by their own peculiar signs and symptoms; and in *pneumonia* is the pathognomonic sign of its *first* stage, disappearing on the supervention of hepatization, and re-appearing with modified characters (*rhonchus crepitans redux*) with the resolution of the inflammation; I regard it as the most practically useful of all the stethoscopic signs, inasmuch as it points out, in its very earliest stage, one of the most severe and most common diseases, and thereby enables the physician to apply his means with much more chance of success than he could have done even a few hours later." But if the word *pathognomonic* is to be construed in its literal sense, leaving entirely out of view concomitant phenomena, and applied to any symptom or sign of disease; so far as we are informed, there are extremely few, if any, symptoms or signs of any affection whatever, that may be so regarded.* With what reason or justice, then, does Dr. Manson single out the *crepitant rhonchus* to undermine its importance, because it is not *absolutely pathognomonic* of—*peculiar* to—*pneumonia*? He speaks of his "*malarial* pneumonia," as a disease distinct and different from other forms of pneumonia, and affirms that it is marked by certain "*peculiarities*" that particularly distinguish it. Has he furnished us with any *pathognomonic* symptom or sign of this "*peculiar*" affection? No—and he cannot do it. Indeed, on the contrary, he declares positively that it is "*often confounded* with simple, typhoid and other varieties of pulmonic lesion;" and "that *many instances* must present themselves to the observer in which *no pathognomonic* guides

* Black vomit was for a long time considered pathognomonic of *yellow fever.* If any one still thinks so, let him consult the great work of Dr. La Roche on this fever, Vol. I. Chap. xii., 264.

will be perceived." Hence it is that he wisely cautions us "*ever to be on the alert, in fear of a mistake;*" and is desirous of "*impressing* on the mind of the beginner that he will *often be compelled* to diagnose and treat *many cases* of which his knowledge must be *chiefly* drawn from the *character of the epidemic present.*" A striking commentary this, upon the great uncertainty of the so-called pathognomonic signs and symptoms! And yet Dr. Manson objects to the *crepitous rhonchus* because it does not "possess *alone*"—isolated, and unconnected with other phenomena—absolute pathognomonic value in the diagnosis of pneumonia! Who ever advised that it should be taken "*alone*"—that other indications should not be considered and duly weighed in making out the diagnosis? Surely not *Lænnec*; for *he* tells us that in certain cases we should have recourse to the "*general symptoms,*" in order accurately to discriminate between the crepitous rhonchus of *pneumonia,* and a similar *rale* occurring in two *other diseases,* it being sometimes difficult to distinguish them "by the stethoscope *alone.*" If Dr Manson's reasoning be sound logic, it is easy to undermine the importance, and to discredit the value of any phenomenon of disease, by simply battling against claims that no body ever set up for it.

If we compare the present state of our knowledge of thoracic diseases with what it was before the subject was illumined by the genius of Lænnec, and consider the means now so well known to us by which we may, in many instances, form diagnoses almost with the accuracy of ocular inspection, we are filled with admiration at the unequalled splendor of his achievements and with gratitude for the great boon he has bequeathed to his successors. But it cannot be expected that he could have discovered and perfected every thing relating to the subject of physical exploration. That would have been an achievement which the pioneer in no other science has ever accomplished. The only wonder is that he did so much ; that during the brief period of ten years elapsing between the first direction of his attention to the subject and his death—in which his labors were frequently interrupted by severe and protracted illness—he should have made so many discoveries of such imperishable value, and have fallen into so few, and often unimportant errors. He doubtless

went too far in the statement that the crepitant rhonchus is "*invariably* present," in the first stage of pneumonia; yet when it presents itself with its distinctive characters perfectly developed, it is certainly very rarely observed, except in the first stage of *pneumonia*, and consequently he did not greatly err in regarding it as *practically* very nearly pathognomonic of that disease.

2. We will now present the views of *Andral*—the second writer upon Auscultation—in respect to the crepitous rhonchus. Let us see how far he differs from Lænnec in regard to its value as an important sign of the first stage of pneumonia, and to what extent, if any, does he lend support to Dr. Manson's opinions of this *rale*.

Andral's opinion of the *crepitous rhonchus*:

"If we auscultate the chest from the very onset of the pneumonia, this is what we observe in the majority of cases. On the side where the pain is manifested, the natural respiratory murmur has lost its clearness; it is mixed to a greater or less extent with a dry *rale*, designated by Lænnec *the crepitous rale*, in consequence of the resemblance between the sound it produces, and the sound which is heard when a salt decrepitates on burning coals. Oftentimes also it has a still more perfect analogy with the particular sound produced by rubbing a bit of parchment.

"During the first moments of the existence of this *rale*, it alters and obscures the natural murmur of respiration, but it does not entirely mask it. According as the inflammation advances, it becomes more and more marked, and finally conceals altogether the respiratory souffle.

"The crepitous *rale* indicates engorgement of the lung; whilst it exists, it is a proof that, in several points *at least*, the pneumonia has not passed the first stage. But from its greater or less intensity, from its greater or less mixture with the natural murmur of respiration, we may deduce signs regarding the more or less advanced state of the first degree, regarding even the union of the first with the second degree. As long as the natural respiratory murmur predominates over the crepitous *rale*, we should infer that the inflammation is slight. If the crepitous *rale* becomes in its turn predominant, if ultimately it altogether masks the respiratory murmur, it is a certain index that the pneumonia has made progress, that it has a tendency to pass to the second degree. When the patients die whilst they present the crepitous *rale* in this degree, we generally find the lung simply engorged, though it be still pervious, its tissue is now softened and friable; it admits of being torn with considerable ease. At a later period still, the crepitous *rale* ceases to be heard; but then two cases may present themselves: either at the same time that the crepitous *rale* diminishes, the natural murmur of respiration is heard anew, or this murmur does not return, and at the same time sometimes nothing is heard; sometimes this murmur is succeeded by another, which we shall speak of presently. In the first case, we must admit that the pneumonia progresses towards resolution; in the second case we attain the certainty that the disease becomes more severe, and that the lung is being hepatised.

The dry crepitous *rale*, such as we have described it, does not always present itself with such marked characters. In several cases it is more moist, and approaches by imperceptible shades to another species of *rale*, which results solely from a mixture of air

and liquid in the large bronchi (the mucous *rale* of Lænnec). On the other hand, we often hear a *rale* entirely similar to the crepitous *rale* in persons who are affected only with an intense bronchitis, and in whom there is found after death *neither* pneumonia, nor pulmonary œdema."(*a*)

In a note to the last edition of Lænnec's work, Andral remarks —in reply to some comments upon his (A's) opinions of this *rale* by Meriadec Lænnec—a cousin of the celebrated discoverer :

"I still think the crepitous rhonchus, although *occurring almost constantly in the first stage of pneumonia*, is not a pathognomonic symptom of this affection. It may be met with in a great many cases, when certainly nothing exists but simple bronchitis, either acute or chronic, when inflammation is seated in the *small ramifications of the bronchi* and they become filled with a viscous fluid, obstructing the transmission of the air. The crepitous rhonchus differs from the mucous rhonchus only by simple shades, and they often approach so near as to be confounded together."(*b*)

The reader has now before him Andral's description of the *crepitous rhonchus*, and his opinion of its importance as a physical sign of the first stage of pneumonia. How different is his appreciation of its value from that which Dr. Manson is pleased to maintain ; and yet this writer is one of the "distinguished authorities" adduced by Dr. Manson to give weight to his disparaging estimate of this *rale*, and to bring it into disrepute! *Andral* says that the *crepitous rhonchus* occurs *almost constantly* in the first stage, and is observed in the *majority of cases from the very onset* of the pneumonia ; that whilst it exists, it is a *proof* that the inflammation has not passed this stage ; that its greater or less intensity even marks the more or less advanced condition of this first degree, determining when the first co-exists with the second, and defining their boundaries ; that if the respiratory murmur predominates over the *rale*, the inflammation is slight ; if the *rale* predominates and masks the murmur, it is a *certain index* that the disease is progressing, and passing into the second stage, that if the patients die whilst they present the *rale*, the lung is found in the first stage, softened and friable ; that when the *rale* ceases to be heard, and the murmur of respiration returns, the pneumonia is progressing towards resolution ; but when it is replaced by another sound—alluding to *bronchial respiration*, which he soon proceeds to describe—it is *certain* that the disease is progressing, and the lung being hepatized.

(*a*) Med. Clinic Vol. II, 187-8.
(*b*) Op. Cit., 222.

Dr. Manson, on the contrary, affirms that this *rale* is *not usually present at the commencement*, or, indeed, *any of the stages of this disease ;* that it does not, of itself, indicate either the seat or extent of the inflammation ; that though *it may* be present, and when taken in connection with other signs and symptoms, affords a valuable aid to diagnosis, yet—as, according to him, no one will gainsay that inflammation of the bronchi is a frequent attendant on pneumonia, making it impossible that sonorous, sibilous and mucous rhonchi should not be often heard, and if present, drowning the less audible sound of crepitation—the first stage is rather indicated by other sounds, viz., abundant bronchial, tracheal and laryngeal *rales*—signs that furnish evidence of bronchitis, tracheitis, and laryngitis, but certainly not of *pneumonia*—and "*in many cases*" that this stage is not revealed by any morbid sound at all! Indeed, we must confess that we are quite at a loss to perceive how Dr. Manson could "consult" Andral's Clinic, and find any countenance therein to his own views respecting the *crepitous rhonchus*, save in the statement that Andral does not consider this *rale* as absolutely pathognomonic of—*peculiar* to, and not observed in any other affection than—*pneumonia*. We have seen already that *Lænnec* only taught that this *rale* is the pathognomonic sign of the *first stage* of pneumonia—the index of the fact that the inflammation is still confined to, and has not yet passed beyond, this stage ; and the reader will please observe that *Andral makes mention of no other auscultatory sign of this first stage*. Lænnec speaks of the "*constant* presence" of this *rale* in the first stage ; Andral says that it occurs "*almost* constantly" ; but that it may be recognised also in *capillary* bronchitis. Dr. Manson quotes from Walshe the well-known fact that the *sub. crepitant* rhonchus of capillary bronchitis, which occurs on both sides of the chest, led Lænnec into the error of unduly magnifying the frequency of double pneumonia—so nearly alike are these *rales* in some cases. Then *Andral* may have mistaken the fine *sub. crepitant* rhonchus of acute *capillary bronchite* for the true *crepitant rhonchus* of pneumonia. And this is the more likely—certainly is just as probable—since at that time the differential diagnosis between these *rales* had not been determined. This was reserved to reward the patient and persevering mind of

Louis, who, according to Fournet, (*a*) pointed out their distinctive characters, (Clinique Medic. de la Pitie, 1836), two years after the publication in England of the latest edition of Lænnec's work, with notes by Andral. That these *rales* resemble each other very much in some cases, and that they may be, therefore, confounded, is very true. This only proves that as they may simulate one another more or less closely, we should be the more particular in studying their diagnostic differences ; and we shall see hereafter, that they may be readily discriminated—at least, almost always. It will be observed that, according to Andral himself, it is only when the *crepitant rhonchus* does not offer its typical characters—when it presents the idea of *moisture* instead of *dryness*—that it approaches by imperceptible shades to the mucous *rale* ; and this is not represented by him to happen except only when the inflammation is seated in the capillary branches of the air tubes (*capillary bronchitis*).

3. *Chomel* is the next author invoked by Dr. Manson to undervalue the *crepitant rhonchus* as a physical sign of the first stage of pneumonia, and to impart to himself more confidence in the candid avowal of his own views in respect to its being not usually present in the course of pneumonia, and its not affording important aid in estimating the extent, and marking the precise seat of the disease. We will now call this "distinguished authority" upon the stand, and endeavor to ascertain to which side of the question his testimony properly belongs.

Chomel's opinion of the *crepitant rhonchus:*

After stating that in the first stage, percussion gives information not much to be relied upon, Chomel goes on to observe that—

"*Auscultation* furnishes information which is more certain; oftentimes, from the very commencement, and even before the expectoration makes us aware of the existence of the disease, it is perceived that the respiratory murmur has lost its strength and clearness; in a short time, the ear when applied to the chest, detects, over a greater or less extent of surface, a particular sound, which seems to be produced by the explosion of a succession of bubbles of air in inspiration, and which has been compared to the sound which is made when salt is thrown upon fire, but which resembles still more that which is produced by compressing the lung of a dead body between the fingers. It is the *crepitant rale ;* the nearer the inflamed part is to the surface of the lung, the more distinct it

(*a*) Clinical Researches on Auscultation, &c. by Jules Fournet, Part I. 120.

is. The bubbles of which the sound is composed are ordinarily very small, following each other in quick succession, almost dry and equal-sized (fine crepitant rhonchus) sometimes, however, and especially in old persons, they are larger, fewer, more moist and less equal in point of size. In some cases, the crepitant rhonchus permits the respiratory murmur to be heard, but feebly ; in others it completely replaces the latter. Over the points where crepitation is not heard, the respiratory murmur is sometimes weakened ; at other times it preserves its natural strength, and sometimes again it is even louder than natural, it is like that which is heard in children ; hence the name of puerile respiration, given by Lænnec to the respiratory murmur in which the strength is increased.

The coincidence of the phenomena observed during life, and of the lesions which exist after death, makes it apparent that the crepitant rale takes place in the spot occupied by the inflammatory process, and that the puerile respiration is heard in the parts remaining healthy. The extent of surface over which crepitation exists, shows that of the inflammation ; and we can, in some cases, follow the increasing or decreasing march of pneumonia, according as the crepitant rale is heard over points where it has not before been observed, or as it abandons others where it has been."(*a*)

The above is what Chomel says under the head of *Auscultation.* We will now turn to his remarks upon *Diagnosis.*

"The diseases, the symptoms of which offer the greatest resemblance to those of inflammation of the lung, and which could be confounded with it, are acute bronchitis, especially that variety which affects the small ramifications of the bronchial tree, pleurisy, œdema of the lung, and certain forms of pulmonary phthisis. But in bronchitis the pain is felt behind the sternum, the cough is less painful, the expectoration is not rusty colored ; at first the sonorous and sibilant *rales* are heard, and the *moist rale which ensues is produced by large bubbles, more unequal, and exist generally on both sides at the posterior and inferior portion of the chest.* In œdema, the pain is absent as well as the rusty colored expectoration, the *rale is produced by larger bubbles, it exists for a longer time with the same intensity,* and the general phenomena belonging to acute inflammation of an important viscus are not observed."(*b*)

In another work, after stating what sounds might be confound ed with the *crepitant rale*, he proceeds to remark that :

" The *pneumonia crepitation*, such as we have described it, *exists in the immense majority of cases of pneumonia* ; *almost always it can be easily distinguished from other respiratory phenomena, which resemble it ; therefore,* WE CANNOT TOO MUCH INSIST UPON THE IMPORTANCE WHICH IT POSSESSES IN THE DIAGNOSIS OF PNEUMONIA."(*c*)

Lastly, in still another work, he states—after describing the *crepitant rhonchus:*

"The rhonchus which presents the above characteristics, *only occurs in the first stage*

(*a*) Dict. de Med., 2nd Ed., (1842) tome xxv, 170.

(*b*) Op. Cit., 196, 197. The curious reader will observe that Dr. Manson refers to " Dict. de Med. *tome xvii.* 232"—the reference being to a previous Ed. The one cited above is the last that has been published.

(*c*) Clinique Medicale, Article Pneumonia, 158.

of pneumonia, and is consequently one of the *most important signs of the disease*."(*a*)

Can it be necessary that we should dwell at length upon the above quotations from Chomel? The reader has, doubtless, already contrasted his views in regard to the *crepitant rhonchus* with those advanced by Dr. Manson. Is it not truly astonishing that *Dr. Manson* should have referred to *this* author to corroborate *his own* opinions of this *rale?* It may well be doubted whether any two observers have differed more widely about any thing. *Chomel* describes the *crepitant rhonchus*, both in respect to its usual typical, and exceptional characters, in language at once lucid and simple; he states that oftentimes, from the very commencement, and even before the appearance of the peculiar sputa, the vesicular murmur looses its normal strength and clearness, and in a short time this *rale* is detected; that the coincidence of the phenomena observed during life, and the lesions ascertained to exist after death, all prove that the *rale* indicates the very spot occupied by the inflammatory process, and determines its limits; that this *rale* exists in the immense majority of cases of pneumonia; and although it may be mistaken for other respiratory sounds which resemble it, yet he is careful to point out real differences between them, and affirms that almost always it can be easily distinguished from them; that *true crepitant rhonchus*, presenting the *characters described by him, only occurs in pneumonia in the first degree*, and therefore, we *cannot too much insist upon the importance which it possesses in the diagnosis of that disease.*

The contrast between these views, and those enunciated by Dr. *Manson*, is as obvious as it is irreconcilable.

4. Next in order, comes Cruvelhier. This author is certainly very distinguished, as a writer upon *Special* and *Pathological Anatomy*. But we, at least, know nothing of his authoritative claims as a writer upon Physical Diagnosis, and of his attainments as a *practical* auscultator. It would seem, however, that

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(*a*) Elements of General Pathology, 3d. Ed., 140. In the very last edition of this work published within less than two years of his lamented death, Chomel repeats this statement: "Le *rale* qui offecdes caracteres qui nous venous d'indiquer n'a lieu que dans pneumonie au premier degree, et constitue par consequent un des signes les plus importants de cette maladie." (Paris, 1856,) Op. Cit., 211.

Dr. Manson is more familiar with his history; but since he has failed to indicate the source from which his information is derived, we are unable to test his reference. We find that Meriadec Lænnec, in a note to his cousin's treatise, makes the following statement: "M. Cruvelhier regards the *crepitous rhonchus* as a sign of no value in pneumonia, *in comparison* with those of bronchophony and tubary respiration"(*a*). *But that is not the question now before us.* We are now discussing the claims of the *crepitant rhonchus as a physical sign of the first stage* of pneumonia. In hospital—and, very generally, also, in country—practice, the disease has already passed into the second stage, more or less when the cases first come under care; and then, of course, the *crepitant rhonchus* has given place to the development of the signs indicative of this stage. "The *great majority of persons* admitted into hospitals," says Walshe, "have some amount of hepatisation when first seen"(*b*); and Flint, also, states that "in a *very large proportion* of hospital cases bronchial respiration is found when patients first come under observation"(*c*). As, then, the disease is generally already advanced to the second stage when the patients are first seen, in hospital practice more especially; and as it very soon passes into this stage and remains in it much longer than it does in the first, bronchial (or tubary) respiration and bronchophony are—being oftener observed—in this sense comparatively of more value than the *crepitant rhonchus*; but that does not affect the value of the latter as a *physical sign of the first stage*. It not unfrequently happens, however, that if a careful auscultation be made within the first few days from the onset, the signs of both stages may be recognized when first called to the cases, unless the pneumonia should happen to be central.

It may be remarked, *en passant*, that *bronchial respiration*, in its turn, is not absolutely pathognomonic of *pneumonia* advanced to the second or third degree; for it may be heard, also, in phthisis, pleurisy, dilatation of the bronchi, and œdema of the lungs.

(*a*) Op. Cit., Forbes' last Ed., 222.

(*b*) Op. Cit., 437.

(*c*) Physical Exploration and Diagnosis of Dis. of Resp. Organs, by Austin Flint M.D., 412.

Of twenty-six cases of pleurisy, selected indiscriminately in the wards of the hospitals *Hotel Dieu* and *La Charite*, at Paris, M. M. Barth and Roger state that it existed in nine cases.

5. Stokes' opinion of the *crepitant rhonchus.*

What is the testimony of *Stokes?* This writer, in his work on Diseases of the Chest, devotes a section to the general principles of the Diagnosis of thoracic affections. He insists particularly on the importance of uniting the study of the rational symptoms and natural history of pulmonary affections, with their physical signs; and of *comparing* the mode of succession of these latter phenomena as they are developed in the career of different lung diseases. He states:

"That the study of symptoms alone cannot lead to accurate distinction of chest disease; the same remark is applicable to that of physical signs, unconnected with symptoms. *There is no such thing as a perfectly pathognomonic symptom or sign of any thoracic disease.* We must combine the lights drawn from the careful study of symptoms both past and present, with the observation of physical signs; for by this mode alone can we hope to arrive at an accurate result. Great injury has been done to the cause of physical diagnosis by some *inexperienced men, who, departing from the principles of its illustrious founder,* have neglected too much the study of symptoms."(*a*)

Here we find it stated by one of his most eminent pupils, that the illustrious *Lœnnec* taught the importance of uniting the study of the vital phenomena, with the physical signs of the disease; and that the science of physical diagnosis has been greatly injured by the *inexperienced,* by departing from the precepts of the great master. In illustration of this principle, Dr. Stokes proceeds to adduce various instances, and insists with emphasis on the importance of applying it in the proper clinical interpretation of the *crepitant rhonchus.* After specifying the diseases in which it *may* be heard, etc., he adds:

"But if this crepitating rale was observed in a patient who had been but 24 or 28 hours ill. and had previously no symptoms of pulmonary disease; if he had inflammatory fever, pain of the side, cough, acceleration of breathing, and viscid expectoration, we might safely conclude that its cause was an acute pneumonia in the early stage."

He might have gone still farther, and have said that the diagnosis would then be positively certain; for who ever saw this concurrence of phenomena in any other disease than pneumonia?

(*a*) Op. Cit., 45.

But while this author insists so strongly—and in some instances certainly carries his illustrations of the principle to an untenable extent—on the importance of combining the study of the vital and physical phenomena, yet, he is "*anxious not to be understood* as *depreciating* the *importance* of studying the *actual characters* of physical signs. On the contrary," says he, "I am *convinced that the more the ear is accustomed to appreciate minute differences of sound*, the *greatest will be our accuracy in detecting the nature of disease*." But while this is true—although he admits that the vital cause of phenomena may be inferred from the character of sounds—that degree of accuracy, he thinks, is not easily attainable, and can be enjoyed only by the few. When he comes to give the physical signs of the first stage of pneumonia, here is what he says:

"The *crepitating rale* and the gradually diminishing vesicular murmur, *constitute the signs of this stage*, and it is the combination of these phenomena which gives them their value. It must be admitted that Lænnec has not succeeded in establishing the crepitating rale as an invariable phenomenon in this disease. It is neither invariable nor positive, but, like all other physical signs, derives its value from the preceeding and accompanying phenomena. As a physical sign, it only points out a secretion or effusion into the pulmonary cells; and, to determine that this is pneumonic, we require the increasing dulness, and gradual obliteration of the respiratory murmur."(p. 277).

In his Recapitulation of the signs of the several stages of pneumonia, he states, (p. 299):

"That in Lænnec's first stage, the signs are, the crepitating rale, the gradually diminishing murmur, and the comparative dulness of sound":

And again, in giving the differential diagnosis between hepatisation of the lung, and pleuritic effusion, he says:

"In the majority of instances the phenomena of voice, *and the existence at all periods of some form of crepitating rale*, in conjunction with bronchial respiration, will distinguish pneumonic solidity from liquid effusion." (p. 474).

Now, what is the proper interpretation of the views of this author respecting the value of the *crepitant rhonchus* as a physical sign of the first stage of pneumonia? We are told in the first place that there is no such thing as a *perfectly pathognomonic symptom or sign*—none positively *diagnostic*—of any thoracic affection whatever; that to this law, the *crepitant rhonchus* is no exception; but still this *rale* is *the* physical sign of Lænnec's first (*his* second) stage of pneumonia, although it is not an *invariable* phenomenon—is not *constantly* present. In other words, though

not always to be recognized; yet, being *the physical sign* of the *first stage*, if this stage be revealed by any auscultatory sign at all, this is the sound to be heard. Dr. Manson, *per contra*, says that this sign is *not usually present at any stage of the disease!*

6. Skoda's opinion of the *crepitant rhonchus:*

Lastly, under this head, we come to Skoda—the great *medical skeptic* of modern times, the man who doubts almost everything—very often, indeed, the testimony of his own senses. As a therapeutist, he has no faith in any remedial agents, save quinine in intermittents, mercury in syphilis, and opium to relieve pain. We subjoin a "pen and ink portrait" of him, by one who has followed his clinical rounds at the great hospital of Vienna:

"That which constitutes the originality of Skoda among all the clinical teachers of Germany, and which has made for him so universal a reputation, in his *skepticism*. In medicine there has been rarely seen, if ever, a *doubter* so absolute, so fervent; for his is no *theoretical* skepticism, (which is a very common thing), but a *practical* skepticism, which he actively propogates, both by his teachings and through the writings of his pupils, and by its application at the bedsides of the sick. Thus, from his name, any physician who neither believes in, nor practices any form of therapeutics, is termed *Scodist*. *Scodism* among the Germans is pyrrhonism in medicine. We would lay long odds that our reader could never divine the remedy which Skoda applies at the bedside. Every year, during nine or ten months of clinical lessons, he employes on his twenty eight sick—*patients* they may indeed be called—*in succession* all the most classical, most celebrated means of cure, and do you know with what intention? Simply to convince his pupils that all those medications are always and completely *inefficient*. If by chance—*chance* is indeed the term to use here—if on any treatment there supervenes a prompt and very marked amelioration, he attributes all the honor to the natural course of the disease.

Example.—A young man of nineteen, very robust, comes into the hospital on the 11th of May, on account of a pneumonia of the right lung, of a highly inflammatory and severe form. On the 13th and 14th, Skoda causes him to take infusion of fox-glove, which induces six stools a day. On the 15th a pound of blood is drawn from the arm by his orders. Next day, the 16th, the pulse, which on the preceeding evening was at 106, falls to 66.

To explain so notable and prompt a modification of the pulse, Skoda expresses himself in these terms: "Perhaps it is the effect of the bleeding, such things have been seen; perhaps, too, it may have been the effect of the fox-glove, that has been seen too; it may also be considered as connected with the natural evolution of the disease, that has been seen too." Skoda reasons habit-

ually after this fashion, never denying in a very decided manner. In this way, little by little, he *insinuates* doubt into the minds of his disciples, all the more surely that he does not insist on its reception, so that finally these come insensibly to lose all practical faith—to raze from their medical vocabulary the word *causality* just as their master does."(*a*)

Such, according to this eye-observer, is the "celebrated Professor Skoda of the school of Vienna." The same habitual proclivity to cavil and incredulity, the same hesitating uncertainty and want of faith manifested in regard to the orthodox truths of therapeutics, he displays in regard to the generally received doctrines of physical exploration. It might be too much to say that the testimony of such a witness against the generally received doctrines of physical exploration, are to be placed upon a par with his testimony against the orthodox truths of therapeutics. Though he cannot see truth in the light in which it appears to other eyes, and has made a great effort to "remodel the doctrines of auscultation at large"—yet his reputation as a writer on this speciality is undoubtedly high, and the results of his observations, when clearly expressed, are entitled to respectful consideration. But as might well have been anticipated in respect to the writings of a man of such mental peculiarities and habits, perspicuity neither of ideas nor of expression, is particularly characteristic; and mis-interpretation of his views, as in the case of German mystics in general, has been brought forward to account for the opposition they have encountered. It is upon this ground, indeed, of the mis-interpretation of his views, that his enthusiastic admirer, the translator and editor of his work on auscultation apologizes for the difference of opinion which prevails as to their value, and very properly observes that "if obscurity attaches itself to isolated portions of an author's reasonings on any particular subject, (and in some rare instances, he admits, this fault may perhaps be objected to Skoda,) it is evident that the reader should endeavor to seek his real meaning in a fair consideration

(*a*) Dr. Gallarvardin, in Verg. Med. Journal, vol. xi., 520-1, from the Edinburgh Med Journal.

of the whole of the argument, and not by partial views of it." In the spirit of this just observation, let us look into what Skoda has said upon the subject in hand. He says :

"I consider Lænnec's crepitation, viz : the fine, equal-bubbling *rale*, denotes the presence of fluid in the finer bronchial tubes, and in the air-cells, and the entrance of air into the latter ; but that it does not inform us by what particular disease the fluid is produced, this must be learned from other phenomena. Moreover, I have not only not found this crepitation constantly present in pneumonia, but if we are to follow Lænnec's description of it very closely, I must say that I have not often observed it." (*a*)

What is Lænnec's description of this *rale?* Skoda quotes Lænnec as follows :

"The moist crepitating *rale* evidently arises in the pulmonary tissue; it may be liken ed to the sound produced by greatly heating salt in a pan or by the inflation of a dry bladder, or, still better, to the sound produced by the pressure between the fingers of a healthy lung distended by air, but it is somewhat louder than this ; and, in addition, it has a well-marked character of fluidity. On hearing it, we at once recognize the presence in the air-cells of a fluid of the consistency of water, by which the entrance of the air into them is not prevented ; the bubbles forming the *rale* appear extremely small"(*b*)

It will be perceived that Skoda says first, that he has not found the crepitant *rale* " *constantly* present in pneumonia." If this expression is to be taken in its precise, literal sense, his experience in this respect but fully accords with the observations of others. There can be no doubt of the fact that this *rale* is never present in certain cases of pneumonia, from their beginning to their close. Skoda, however, proceeds further to state, that if we are to follow *Lænnec's* description" of this *rale* " *very closely,*" he must say that he has " not *often* observed it." But what if he does *not* follow *Lænnec's* description of it "*very closely*"—are we to understand that he does then observe it? Who can say? When he says he " rarely meets with it," he is commenting on Lænnec's description of it. (*c*) Now, if we examine "very closely" Lænnec's description of this *rale*, we shall see that in the early and more imperfect state of the Science of Auscultation, and of the discoveries made by it, Lænnec did not seem to have distinguished accurately all the characteristics of this *rale*, nor at all times have maintained or expresssed clearly the same views in respect to one, at least, and a very important one, of those characteristics. We find, however, that even here his nice observation and

(*a*) Op. Cit., 156-7.
(*b*) Op. Cib., 155.
(*c*) See the quotation from Skoda, already made.

acute perception led him in the direction of truth, and that although he had before—as in the quotation cited by Skoda—indicated a different opinion, he distinctly states, in describing this *rale* as it appears in pneumonia, that it "seems *hardly* to possess the character of *humidity*." Subsequent observers have established the fact, that it generally does not possess that character at all, and thus have brought into full light the truth, of which Lænnec lingered on the verge. Those authors have also established, that the difference between the crepitant and the subcrepitant *rales*, is sometimes extremely nice, and, "judging by its intrinsic characters" alone, not always easy of prompt and precise recognition. Now, if in saying that, should Lænnec's description of the crepitant *rale* be following "very closely," he did "not often observe it," Skoda merely meant to say that he did not in the first stage of pneumonia often observe a "fine, equal-*bubbling rale*," of which "*fluidity*" is a distinguishing characteristic, he has said no more than Lænnec's later intimation in respect to that characteristic would fully warrant, and no more than what all subsequent eminent auscultators have verified and maintained.—In this view, Skoda's testimony only yields incidental and implied, but not the less strong and striking confirmation of that cautious intimation of Lænnec, and of the clearly established teachings of more recent observation. It is justly remarked by Flint, that even those auscultators at the present day, who attribute the sound of this *rale* to bubbles, nevertheless consider *dryness* as one of its distinctive features. Thus, Walshe, who rather inclines to adopt the bubbling theory (originally taught by Lænnec) as appearing to him to be most probably the true one, yet insists that "*dryness* is one of the most *marked properties* of true crepitation; the sensation is not that of bubbles breaking, but rather of delicate tissue undergoing minutest ruptures with a crackling noise in many points simultaneously.(*a*) Hence, it does not follow, that because Skoda says "the fine, equal-*bubbling rale* denotes the presence of fluid in the finer bronchial tubes and in the air-cells, and the entrance of air into the latter," he means to say that this *rale* of necessity imparts to the ear the idea of "*flu-*

(*a*) Op.Cit., 112.

idity;" for other auscultators who adopt the theory that the sound is caused by the bursting of minute bubbles in the air-cells and terminal tubes, insist that "*dryness*," and not "*fluidity*," is one of its most prominent properties.

Again: Skoda quotes Lænnec as stating that "the bubbles forming the *rale* appear *extremely small*." It may be that it is to this—the extreme minuteness of the bubbles, of which he considers the *rale* to be composed—that he refers, when he says he has "not *often* observed it, if we are to follow *Lænnec's* description of it *very closely*." Now, although the crepitations forming this *rale* are generally fine—while this is the rule—yet they are not always so. "There is something so peculiar," says Fournet, "in the crepitant rhonchus of pneumonia, in its fine, dry, regularly rounded, uniform bubbles, closely approximated to each other, and decrepitating in rapid puffs beneath the ear during inspiration, that I do not think there is any known rhonchus that can perfectly simulate this assemblage of characters, when they exist in the marked form I have just described; accordingly, in such circumstances, the crepitant rhonchus ought, in my opinion, to be considered pathognomonic of pneumonia; *but it does not always present itself in this well marked form*. In this case the rhonchus of acute bronchitis (the *sub-crepitant rhonchus of acute capillary bronchitis*) is that with which it may be most easily confounded." (*a*) Valleix says, in speaking of the physical signs of pneumonia: "Amongst the stethoscopic signs, the only one which is truly pathognomonic is, as has been demonstrated by M. Grisolle, the *crepitant rale*, which it is necessary to distinguish well from the *sub-crepitant rale*. When it is heard there can be no doubt; its very small, dry, equal-sized bubbles assure us of the existence of pneumonia; but the *rale* does not always present characters so exact; *often it resembles the sub-crepitant rale*, and may even be confounded with it. It is necessary, then, to have recourse to other circumstances to determine its value with precision."(*b*)—"There is not a great difference," remarks Louis, "between a *crepitant rale*, rather coarse, (for it is not always equally fine) and

(*a*) Op. Cit., Part I. 120.
(*b*) Guide du Med. Prac., P. I, 443.

a *sub-crepitant*, rather fine."(*a*) In 34 cases of pneumonia observed by Grisolle, the primary crepitation resembled more or less closely the sub-crepitant rhonchus of acute capillary bronchitis.

There can be no doubt, then, that the crepitant *rale* is not always composed of crepitations that are "extremely small" and equal-sized; and it is under these circumstances that the sub-crepitant *rale* has been confounded with, and mistaken for it; and *vice versa*. Indeed, it would seem that Skoda himself has done this very thing; or, at all events, that the *rale* which he oftenest observed in the first stage of pneumonia, partakes more of the character of the sub-crepitant, than the crepitant *rale*, for he says in another place—while commenting on Lænnec's account of the auscultatory signs of pneumonia: "It frequently happens that the fine equal-bubbling *rale* is not heard at the commencement of pneumonia; we meet with the *unequal* bubbling *rale*—Lænnec's mucous *rale*—or whistling and sonorous sounds, *much more often* at this period." And further on he adds: "It would be hard to say, whether the *rale* which is heard *previous* to the hepatization, or the consonating *rale* attendant upon complete hepatization, has been most frequently taken for Lænnec's crepitating *rale*.(*b*) As Skoda's consonating *rale* is said by him to be "of the same significance as bronchial breathing and bronchophony"—that is, when it occurs in pneumonia, it indicates hepatization—it could not present itself in the *first* stage of pneumonia. We are, then, thrown back upon Lænnec's unequal bubbling *rale*; and as no one could mistake a large *mucous* for the much finer *sub-crepitant rale*, and certainly not sonorous and sibilant rhonchus for either, it is to the *sub-crepitant rale* that Skoda must refer, as being much more often met with by him at the commencement of pneumonia, than the fine equal-bubbling *rale*—the true *crepitant rale*. So that, it seems, we must come at last to the conclusion, that Skoda meant to say that, a *rale* presenting the characters of the *sub-crepitant*, is much oftener met with at the commencement of pneumonia, than one offering the typical characters of the *crepitant rale*; or, in other words, that the crepitant *rale* in the first stage of pneumonia is not, as a rule,

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(*a*) Researches on Blood-Letting, &c., 51.
(*b*) Op. Cit., 321-2.

composed of very fine equal-sized crepitations, but offers rather, crepitations that are larger and not all of equal size—the sub-crepitant *rale*. This may be, perhaps, after all, the true key to Skoda's real meaning, when he says he has "not *often* observed" the crepitant *rale* in pneumonia, "if we are to follow *Lænnec's* description of it *very closely*." Thus interpreted, the statement might have struck auscultators with less astonishment.

In regard to the *clinical* value of the crepitant *rale*, Skoda alleges that it indicates "the presence of some fluid—mucus, blood, or serum—in the finer bronchial tubes and in the air-cells, and the entrance of air into the latter; but that it does not inform us by what particular disease the fluid is produced." Hence, it cannot be strictly regarded as *pathognomonic* of any disease. But in what other disease than *pneumonia* does he profess to have observed it? In *œdema* of the lungs, he says it "*may* occur"; yet that is all. Nothing is said about its frequency. And although he represents this *rale* as denoting the presence of *blood* in the air-cells and minute bronchi, yet he declares, "I have *very many times* sought for this crepitating *rale* in cases of *hœmoptysis*, but have *rarely found it*." Indeed, he says that "this abnormal condition of the lung parenchyma,"—Pulmonary Apoplexy, "is *rarely met with*, and very few cases of hœmoptysis depend upon it." Again he affirms: "It is *possible*, that the crepitating *rale* may *occasionally* be associated with *hæmorrhagic* infiltration, but I have *always* hitherto observed some *other kind* of *rale* or whistling or sonorous sounds. (*a*)

We have thrown out the above suggestions in regard to the seeming rarity with which Skoda has observed the crepitant rhonchus in pneumonia, as offering a possible explanation of the apparent conflict of his language with the concurrent testimony of the best modern writers on the subject. We confess that to us there is some obscurity as to what is his precise meaning. If its true interpretation be that above indicated, Skoda's testimony against the crepitant rhonchus as a valuable sign of pneumonia, is divested of much of its ostensible force. But if this interpretation be incorrect, and that placed upon it by Dr. Manson be

(*a*) Op. Cit. 325-6-7.

the true one—which certainly is the received construction—of what weight is Skoda's isolated observations—the negative testimony of one individual—to have in the balance of Science, when opposed to the positive testimony and concurrent experience of a host of auscultators, at least as observant, as skilful, and as eminent as himself; men of safe and solid intellects, trained to the highest possible degree of acuteness and accuracy in the best and widest fields for close and continued observation? Moreover, is there any very precise idea in the expressions used by Skoda in regard to the infrequency of occurence of the crepitant *rale* in pneumonia? "I have not *often* observed it"! "I *rarely* meet with it"! What is the proper construction of this language? Does he meet with this *rale* five, ten, fifteen, twenty, twenty-five, thirty, or forty times in a hundred cases? What is the experience of those who have tested this matter by the numerical method? Of two groups of cases observed by Louis—78 in one, and 29 in the other—amounting to 107, in which he analized the effects of bloodletting on the rational symptoms, physical signs, &c. of pneumonia, he speaks of the diagnosis being confirmed by *crepitation*, resonance of voice, dullness on percussion, &c, and does not note a solitary case in which *crepitation*—one of the synonymes of the crepitant rhonchus—was absent. (*a*) Aran failed to discover it in only *one* of *fifty* cases. "Out of forty-four cases," says Flint, "taken in regular order with a view to an analysis of the recorded physical signs, in *thirty-two* a crepitant *rale* was observed and in twelve its presence was not noted. But of these twelve cases, in eight a single examination only was made, and in all at a period more or less remote from the commencement of the disease. It is probable that examinations repeated, and made at an earlier period, would not have been negative as regards this sign in the greater proportion of the few instances in which it was not discovered."(*b*) What says Grisolle, the highest of all authorities on the question in hand? He states that "the crepitant *rale* characterizes pulmonary engorgement, or the first degree of pneumonia: it is a sign the more important to discover, since it exists

(*a*) Op. Cit. 12-25.
(*b*) Physical Exploration, &c. 409.

ordinarily from the very commencement of the disease, and because it is, in almost every case, the first stethoscopic phenomenon which can be discovered. We have heretofore observed that sometimes it is preceded, during several hours, and during one or several days, by a diminution in the intensity of the respiratory murmur, and sometimes also, according to Stokes, by puerile respiration, but it is not long in succeeding to these first phenomena. To give an idea of the frequency of the crepitant *rale* in pneumonia, and to have on this account an idea of its symptomatic importance, it will suffice for me to say that it may be assumed as a kind of *pathological law*, that, when pneumonia is revealed by any stethoscopic sign whatever, *crepitant rhonchus is the most constant phenomenon;* sometimes it occurs at the very commencement as the primitive symptom; at other times it is consecutive to other signs furnished by auscultation. Up to the present time, I have only seen *four* cases of pneumonia in which I am certain that the crepitant *rale* did not occur at any stage of the disease; in the other patients the crepitant *rale seemed* to be absent at the commencement of the disease, and only occurred consecutively to the phenomenon which we will soon study under the name of bronchial respiration."(*a*) Grisolle's work is based upon the analysis of 373 cases: hence, the crepitant rhonchus was absent only once in 93 cases. Well, then, might Flint exclaim in regard to Skoda's statement of the infrequency of occurence of this *rale* in pneumonia, "this is one of the *extraordinary assertions* enunciated by that writer."

A recent philosophic writer has asked, (with ill-suppressed derision), " Have not sages arisen in our day to deny even the principle of gravity, for which we had been so long contentedly taking the word of the great Sir Isaac?" But we suspect that the universe of worlds around us will continue to move on sublimely, in obedience to that great law, the vagaries of isolated sceptics to the contrary notwithstanding!

If the reader desires any further evidence of the frequency of occurrence, and great value of the crepitant *rale* as a physical sign of pneumonia, he may easily find it.(*b*)

But if it be granted that Dr. Manson is sustained by Skoda in respect to the infrequency of occurrence, and want of diagnostic value, of this *rale* in pneumonia, it is, on the other hand, beyond

(*a*) Op. Cit., 234.

b) Gerhard, Diseases of the Chest, 2nd Ed., 138; Swett Op. Cit., 101; Hughes, Aus. and Phis. Diag., 188; Barlow, Prac. Med., 189; Ellistson, Prac. Med., 753; Watson, Sec. Prac. Physic, 563; Wood, Prac. Med., Vol. II, 12; etc., etc.

all manner of doubt, that he cannot quote this author as stating that this is particularly the case in *bilious* or "*malarial pneumonia*"; for, as we have already seen, the Viennese Professor ignores and repudiates the very existence of that division of the disease, and declares that it "*is altogether based upon theory.*" It is impossible, also, for Dr. Manson to get any help from *Skoda*, even by inference, in making out a "form of *pneumonic lesion*" different from that described by Lænnec, simply on account of the former's differing from the latter in respect to the clinical value of the crepitant rhonchus. Skoda has most completely closed the door in the face of any such "*supposition.*"

IV. We have now examined the opinions of all the authors referred to by Dr. Manson to sustain him in under-valuing the crepitant rhonchus as a sign of the first stage of pneumonia, and to combat the views of Lænnec. On the other hand, as disagreeing with himself, and sustaining Lænnec, he instances Williams, Walshe, Barth and Roger; and represents the "views" of these authors as "conflicting" with those of the authors (Andral, Chomel, Cruvelhier, Stokes and Skoda,) whose opinions we have just examined. Let us now look at this side of the picture. It will be entirely unnecessary for the purpose before us, to quote all of the descriptions of the crepitant rhonchus, given by the authors to be examined, as we have simply to show that they do not represent this *rale* as *absolutely pathognomonic—peculiar* to, and *diagnostic* of *pneumonia.*

1. Williams' opinion of the *crepitant rhonchus:*

After describing this rhonchus, as if on purpose to exclude the conclusion that it is *pathognomonic* of—does not occur in any other affection than—*pneumonia*, he adds the following lines:

"It is agreed on by *all auscultators*, that *œdema of the lungs* and the margins of *hæmoptoic* engorgements may produce a rhonchus of the *crepitating* kind."(*a*)

Should any doubt arise in regard to the exact meaning of the phrase—"a rhonchus of the *crepitating* kind"—it will be removed by looking at the citation we shall now make from a more recent work of this author:

"Crepitation heard in the posterior regions of the chest, is a physical sign of the engorgement of the lungs, with liquid in the minute tubes; but we must *refer to the vital symptoms* to determine whether the engorgement is *inflammatory*, or only *congestive.*"(*b*)

(*a*) Cyc. Prac. Med., Vol. III, 606.
(*b*) Williams' Principles of Medicine, 3d Amer. Ed., 343.

It is very clear from these citations, that *Williams* does *not* insist on regarding the crepitant rhonchus as a sign *peculiar* to—*pathognomonic* of—*pneumonia*. On the contrary, he states most distinctly that a *crepitating* rhonchus is admitted by *all auscultators* to occur in *œdema* of the lungs, and about the margins of *hœmoptoic* engorgements; and that we must take into consideration the vital phenomena of disease, before we can determine the character of the affection—whether it be simply congestive or inflammatory in its nature—when we hear *crepitation* (which, as every one knows, is one of the synonymes of the *crepitant rhonchus*) in the posterior regions of the chest. Indeed, this writer is particularly distinguished for insisting upon the great importance of associating the study of the physical signs with the vital symptoms, and of reasoning on their connection and succession in the just interpretation of clinical phenomena. In proof of this statement, we subjoin the following quotation, which, from the soundness and practical value of the doctrines inculcated, has been frequently cited by subsequent authors:

"It has always appeared to us, that the more the student in auscultation holds in view the *pathological state on which the signs depend*, rather than those signs themselves, and habitually reflects on their physical mechanism, as far as it is known, without empirically dwelling on names or bare descriptions of sound, the more surely will he estimate the value of this method of diagnosis. He will thus see that central peripneumony may be so situated as to yield sometimes no physical symptom, and at others though to be discovered only by a very careful examination; and hence he will see the *impropriety of a partial method* of diagnosis, and the *great importance of attending to the sputa and other indications*."(*a*)

So far, then, as *Williams* is concerned, the matter seems too plain for argument. The reader cannot fail to be reminded of the coincidence of these views in respect to the true principles of exploration with those of Stokes, with which, as "*conflicting*," they are placed in contrast by Dr. Manson! How Dr. Manson could come to the conclusion that *Williams*—who had taken so much pains to express opinions so entirely antagonistic to such a "supposition"—considers the *crepitant rhonchus* as a sign *positively pathognomonic* of—*peculiar* to, and not heard in any other affection than—*pneumonia*, is, we must confess, past our comprehension.

(*a*) Cyc. Prac. Med., Vol. III, 607—8.

2. Walshe's opinion of the *crepitant rhonchus:*

Having described the distinctive characters of the *crepitant rhonchus* in a previous page, Walshe, when he comes to give the physical signs of the first stage of pneumonia, holds the following language:

"In the great majority of cases, watched from the outset, *crepitant rhonchus* is more or less extensively heard during this stage; but, in hospital practice especially, the period of its existence has often passed when the patient is first seen There are instances, too, in which, though the pneumonia be developed under observation, *no crepitation occurs;* here the intra-parenchymatous exudation seems to be thrown out so rapidly, as to arrest all breathing action in the actually inflamed part, *ab initio.* And in a word, the diagnosis of pneumonia must often be made without the help of true crepitant rhonchus."(*a*)

In another place he says:

"I long indeed, with Lænnec, believed *fine* crepitation distinctive of pneumonia, but unfortunately in rare instances (I have seen two such within the last year) pure œdema of the lung does give crepitant rhonchus in no point distinguishable from that of pneumonia I have not myself met with true crepitation in pulmonary apoplexy, or in the common hæmoptysis of phthisis." (p. 114.)

It is certainly not a little singular that Dr. Manson should have cited this author as *accurately* verifying the experience of Lænnec in regard to the frequency of occurrence, and pathognomonic value of the *crepitant rhonchus* in pneumonia. *Lænnec* states that the crepitant rhonchus is *constantly present* in the first stage of pneumonia; while *Walshe*, on the contrary, tells us that the diagnosis of this disease must often be made without the help of this *rale*. And in respect to its *pathognomonic* value, he says most distinctly that, with Lænnec, he long believed the crepitant rhonchus, when the crepitation is *fine*, distinctive of *pneumonia*, but that in some *rare* instances of pulmonary *œdema*, it may be heard presenting the pneumonic character.

Walshe's views of the *crepitant rhonchus*, as we interpret them, may be thus stated: Although this *rhonchus* is the one that we must search after as the sign that indicates the first stage of pneumonia, and which will be discovered in the great majority of cases, if they be watched from the outset, yet, as we encounter them, more especially in hospital practice, when the patients have some amount of hepatization when first seen, and the period of its existence has then often passed by, and in certain instances, in which, owing to the rapid pouring out of exudation matter in the pul-

(*a*) Op. Cit., 412.

monary vesicles, and consequent speedy solidification of the lung; all breathing action in the actually inflamed part is arrested. *ab initio*; the diagnosis of pneumonia must often be made without the assistance of this *rale*.

3. The opinion of Barth and Roger respecting the *crepitant rhonchus*.

Lastly, under this division, we come to M. M. Barth and Roger —the authors of the best French treatise on the subject of Auscultation. After describing the *characters*, *differential diagnosis*, *physical causes*, and *analytical diagnosis* of the *crepitant rale*, in accordance with the plan of their work in treating of all the physical signs, and stating that this *rale* occurs in pulmonary congestion, (active and passive,) œdema, pulmonary apoplexy, and, above all, in *pneumonia* with its characters most developed, they finally come to its semeiological value—the conclusion—as follows:

"On account of the extremely frequent occurrence of inflammation of the lung contrasted with the comparative rarity of œdema and apoplexy, crepitating *rale*, especially when its characters are well developed, is a sign, almost pathognomonic of pneumonia at the period of congestion." [*b*]

It cannot be necessary to add a word of comment on the views of these authors. While they attach great importance to the *crepitant rhonchus* as a sign of pneumonia in the first degree, and state that in this disease it oftenest presents its typical characters, and is more frequently met with on account of the extreme frequency of the disease, yet they state that it may occur in others, which they specify under the head of the analytical diagnosis, and of which they take the greatest pains to point out the distinctive characteristics. Could any one, by reading Dr. Manson's representation of these views, have formed any correct idea of what they actually are? Indeed, had Dr. Manson reversed his array of authorities, and referred to those under this head (Williams, Walshe, Barth and Roger,) on the opposite of the question, he would have made a better show; and yet with how little semblance of reason even these might have been cited in support of his peculiar views, he seems to have been fully conscious by citing them against himself.

(*b*) Pract. Treat. on Auscultation, Edinburgh Ed., (1842), 97

V. We have now passed in review all the authors referred to by Dr. Manson in this connection; and what have we seen? It appears to us that the following propositions may be considered as established:

1. That there is no proof, either from their descriptions of the anatomical characters, or their account of the physical signs of the disease, that any of these authors had reference to a "form of *pneumonic lesion*" different from that described by Lænnec; and, consequently, that Dr. Manson's "supposition" that they did, is, (to use the expressive language of Skoda,) "*altogether based upon theory.*"

2. That all of them, with the single exception perhaps, of Skoda, attach great importance to the *crepitant rhonchus* as a physical sign of the first stage of pneumonia; and that Skoda flatly denies even the existence of the form of pneumonia which Dr. Manson describes!

3. That while all of them, with the exception aforesaid, regard this *rale* as an important physical sign of pneumonia in its first stage, they do not differ very widely in respect to the *frequency* of its occurrence; Lænnec speaks of its *constant* presence in this stage; Andral, of its being almost constant; Chomel, of its being met with in the *immense majority of cases*; Stokes, as, although not *invariably* present, still *the sign to be sought for;* Williams, says nothing of its frequency, farther than that its presence is *much more constant than the peculiar expectoration*, and that it is the *more available diagnostic sign*;(c) Walshe alleges that it is heard in the *great majority of cases, watched from the outset*; and Barth and Roger, whilst saying nothing of its frequency, yet notice it as a sign, when its characters are well developed, almost pathognomonic of *pneumonia* at the period of congestion.

4. That none of them consider this *rale* as in the *strictest* sense *pathognomonic* of—*peculiar* to, and not heard in any other affection than—*pneumonia*; but all of them admit that it—or a rhonchus so nearly analogous as to be sometimes almost, if not quite, undistinguishable by its auscultatory characters alone—may be heard in other diseases.

(c) Cyc. Prac. Med., Vol. III, 616.

5. That it follows of necessity, if *Dr. Manson* observed a "form of *pneumonic lesion*" different from that described by Lænnec, he must look to other sources for the proofs of its existence.

We desire *not* to be understood as intimating in any manner that Dr. Manson could, even by possibility, have intended to misrepresent the above named authors. On the contrary, we have no idea that he had any such intention. But, in view of this exhibition, *in extenso*, of what these writers *actually said* upon the subject in hand, it is submitted to the reader whether Dr. Manson has himself very closely followed the golden rule he commends for the guidance of others—"Observe! Compare! Record!"—and has very carefully "observed" and "compared" in their respective texts, the "recorded" opinions of the authors to which he refers. Indeed, we think it would be difficult to find, in the whole scope of medical literature (so far certainly as our knowledge extends,) an author's statements, and his references to support them, at so complete, absolute, and irreconcilable variance. A perpetual wonder rises in the reader's mind, how Dr. Manson could have examined the authors to which he refers, without having seen that so far from sustaining the positions for which he cites them, there is not only no foundation for such a fancy, but that they are first made by him to entertain views which they have not expressed, and then brought into "conflict" to be "reconciled" only by the "supposition" that they are speaking of different things, and, therefore, that no such "conflict" in fact exists.

We might well rest here this branch of the subject; but as Dr. Manson has thought proper to *argue* the matter, we shall proceed to examine the validity of his logic.

VI. Dr. Manson states that:

"If the peculiar variety of pneumonia now under consideration, or if this phrase is objectionable, if the condition of the organism with which inflammation of the substance of the lungs in the present disease is found associated, is widely different from that described by the majority of authors, then it is reasonable to suppose that some diversity in the physical signs would be observed. If, in this disease, as we shall contend, the pathological condition of the lung usually partakes more of the character of congestion, than open inflammatory action, then the absence of crepitation may be explained, without impeaching the testimony of those who have sought to affix it to the first stage of pneumonia as the diagnostic sound."

If we correctly apprehend Dr. Manson's meaning in this quotation, he seeks to reconcile a supposed material difference on the part of the writers whose views we have just examined (in regard to the value of the *crepitant rhonchus* in the diagnosis of pneumonia), without impeaching their testimony, on the ground that their observations were based upon the study of different forms of the disease; and that the pathological condition of the lungs in the pneumonia of which he treats, "is widely different from that described by the majority of the authors." He goes upon the supposition that the pathological condition of the lungs in the first stage of simple, and "malarial," pneumonia, differs in such a manner as to modify the character of the physical signs.

1. We remark, in the first place, that Dr. Manson takes for granted, *in limine*, what we have shown to have no foundation in fact—viz., that the authors in question offer such a difference in respect to their opinions touching the value of the *crepitant rhonchus* as a physical sign of the first stage of pneumonia, as he has assumed.

2. Dr. Manson again begs the question in this: that, instead of furnishing *anatomical proofs* of his pathology, he is content to base it upon the very uncertain hypothesis of mere clinical inferences.

It is incumbent upon *Dr. Manson* to point out clearly, to give some *reliable evidence* of the difference which he alleges to exist in the pathological condition of the lungs in simple and "malarial" pneumonia. It appears to us that the only difference between simple inflammatory, and non-inflammatory congestion of the lung, is just the difference between the first stage of pneumonia, and a simple stasis of the blood. Now, if the condition of the lung in the first stage of Dr. Manson's "*malarial* pneumonia" be not that of *inflammatory congestion*, then it is not *pneumonia—inflammation* of the pulmonary parenchyma—at all; but if the congestion be *inflammatory*, what difference is there between it, and the first stage of ordinary simple *pneumonia?* Dr. Manson asserts that "the anatomical character of this affection (malarial pneumonia) is doubtless an *intense congestion* of the parenchyma, associated, perhaps, in some degree, with *inflammation*." Well, if there be *inflammation* enough in "this affection" to entitle it to

be properly designated as *pneumonia,* it must be, we should suppose, *par excellence,* a *congestive* pneumonia. But do not all pneumonias commence by congestion; and what is a congestive *pneumonia* in its first stage, but an *inflammatory* engorgement? Indeed, what are the anatomical diagnostic differences between a simple stasis of the blood (simple congestion), and inflammatory congestion? If two specimens of lung, one affected with *simple,* and the other with *inflammatory* congestion, were presented to Dr. Manson, and he were required to indicate their differential diagnosis by their physical characters, we are strongly inclined to suspect that he would be puzzled no little. This matter has not failed to arrest the attention of pathologists. What is their testimony?

Watson says the two kinds of engorgement can scarcely be distinguished from each other by their anatomical characters alone. We judge, says he, by the *situation* of the engorgement sometimes: if it be not in a depending part of the lungs, it is surely inflammatory. But, he adds, after describing the first stage of pneumonia, such a state of the pulmonary substance as I have been describing, you will meet with *in half, at least, of the dead bodies you may have to examine.* (*a*) Williams says he has never been able to discover any physical difference between the first stage of marked inflammation and the engorgement of mechanical congestion. (*b*) Swett says that after the most careful examination, he has been compelled to acknowledge the appearances as identical, and, as we have already seen, refers to Cruvelhier—whom Dr. Manson adduces as authority to sustain him in disparaging the *crepitant rhonchus,* and to make out a form of pneumonia different from that described by Lænnec, as maintaining that the two conditions *are identical in nature and appearance.* Andral once taught that increased *friability* denoted inflammation, but subsequently gave it up. (*c*) Rokitansky distinguishes inflammatory congestion by the *color tending to a brownish red,* and the *moisture* of the parenchyma, which he thinks is sufficient

(*a*) Sec. on Prac. Physic, 3d Am. Ed., 560.
(*b*) Cyc. Prac. Med. Vol. III, 596.
(*c*) Med. Clinic, Vol. II., 176.

of itself to distinguish them, by the special circumstance of its depending on the tissue being filled with blood that has already undergone the inflammatory metamorphosis, or, in other words, with a brownish or brick-red, thin but viscid fluid mixed with black, crumbling flocculi(*d*). But, our distinguished countryman, Dr. J. Da Costa—"whose contributions to pathological science," says Dr. Gross (high authority), "have placed him in the front rank of the pathological anatomists of this country"(*c*)—in the important paper which he has presented on the minute pathological anatomy of acute pneumonia, already referred to in a previous page, has proved that, to the unassisted eye, there is no difference at the outset of the inflammation between the effusion of the first stage of pneumonia, the ordinary serous effusion occurring in œdema, and the fluid exuded in simple congestion of the lung. As the pneumonia advances, the secretion becomes thicker and redder, until it ultimately approaches to the well-known rusty-colored exudation. Neither are there, at first, microscopically, any very material distinctions. The fluids of both contain a few granules, *whilst the structure of the lung is exactly similar*, excepting that the walls of the air vesicles are generally more granular in inflamed than in simply congested portions of lung. In a more advanced stage of inflammatory engorgement the appearances are decidedly changed; there are a larger number of exudation corpuscles, &c. So that, Dr. Da Costa concludes from his observations,—that the number and variety of exudation corpuscles, and perhaps the absence of blood corpuscles undergoing pigmental changes, *alone afford any evidences of acutely—inflamed pulmonary tissue.*"(*a*)

It is apparent from this presentation of the subject, that pathologists have not been able to discover any thing specially distinctive of the inflammatory engorgement of the first stage of pneumonia, from simple non-inflammatory congestion, as regards the physical condition of the lung substance. Rokintasky dis-

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(*d*) Pathological Anatomy, Vol. IV, 65.

(*c*) Elements Path. Anat., by S. D. Gross, 3d Ed., 5.

(*a*) Amer. Jour. Med. Sciences, N. S., Vol. XXX, 295 et seq.

covers only a difference in *color* (*a*), and a certain degree of *moisture*, due to the tissue being filled with a brick-red, thin but viscid fluid, in inflammatory engorgement; but De Costa has proved, as we have just seen, that this fluid differs from that effused in simple congestion by its *microscopic characters alone.*

But we cannot perceive why Dr. Manson should have endeavored to explain a supposed important difference on the part of the authors whose opinions respecting the value of the crepitant rhonchus as a sign of the first stage of pneumonia, he endeavors to reconcile without impeaching their testimony by predicting certain unproved "widely different" conditions of the lungs in the two pathological states assumed. For he soon proceeds to advance two distinct and different theories for explaining the mechanism by which this *rale* is produced, one of which entirely relieves him of any such necessity. Here they are:

VII.

"The presence of crepitating *rale* is *justly and reasonably* ascribed to the passage of air through *the* viscid *pathognomonic* secretion in the air-cells and minute bronchi; *that secretion being considered the result of inflamation.*"

Trans. of the Eight An. Meet. of the Med. Soc. of N. C., p. 49.

"I *believe* crepitation is *neither pathognomonic*, at least of this (malarial) variety of pneumonia, or *confined to inflammatory affections* of the parenchyma, being simply the sound produced by the passage of air through A viscid fluid in minute tubes."

Trans. of the Eight An. Meet. of the Med. Soc of N. C., p. 50.

1. If we are to accept the theory put forward in the *right* hand column, viz: that the crepitant rhonchus is "produced by the passage of air through A viscid fluid," and that this *rale* is *not* "confined to *inflammatory* affections," then, what would Dr. Manson gain, even were it proved that in the first stage of his "malarial pneumonia" the engorgement is not of an inflammatory character? Have we not seen already, and that, too, by the testimony of authors adduced by Dr. Manson to prove just the reverse!—that the crepitant rhonchus may occur in diseases *not* inflammatory, —in *œdema*, and simple *congestion* of the lung, both active and passive? Dr. Manson assumes first that, in "malarial pneumonia,"

(*a*) In speaking of the characteristics of the first stage of *inflamed* lung, Da Costa remarks: "that the *colour*, although darker and more violet than that of the normal tissue, affords no distinguishing mark between the cadaveric and other engorgements, *is pretty generally conceded*"

the "pathological condition of the lung usually partakes more of the character of congestion, than open inflammatory action,"—that there is "an intense congestion of the parenchyma, associated, perhaps, in some degree, with inflammation,"—and then labors to prove that the alleged "absence of crepitation in the large majority of cases may be *thus* explained"; and yet, on the very next page, expresses his "*belief*" that this *rale* is *not* "*confined* to *inflammatory* affections"! Why, then, seek to explain the alleged absence of the *rale* by the assumption that *it is* "confined" to an *inflammatory* affection? Dr. Manson concedes, however, that in "malarial pneumonia" the "intense congestion of the parenchyma" is "associated, perhaps, in *some degree*, with *inflammation*,—an admission at once fatal to the hypothesis of its absence, which his argument requires. It seems to us that "an *intense congestion*" of the pulmonary parenchyma, "associated with *some degree* of *inflammation*"—that is to say, a *congestive pneumonia, par excellence*, in its first stage—offers just the very condition of things, it matters not which of the above theories be adopted, in which we might rationally expect to hear the crepitant rhonchus developed in full perfection.

2. If we now revert to the theory advanced in the *left* hand column to explain the mechanism of the crepitant rhonchus, viz: that it is produced by "the passage of air through *the* viscid *pathognomonic* secretion in the air-cells and minute bronchi—that secretion being considered the *result* of *inflammation*"—then it follows as a logical sequence that this *rale* must indicate an *inflammatory* condition of the pulmonary parenchyma. It thus appears that, according to *Dr. Manson*, the crepitant rhonchus *is*, and *is not* diagnostic of inflammatory affections of lung parenchyma,—a complete solecism! It will not do for Dr. Manson to say that the theory adopted in the *left* hand column above in explanation of the mechanism of the crepitant rhonchus is not his own, but that advanced by some other writer; for he states positively that this *rale* "is *justly and reasonably*" so "ascribed"; and even were it otherwise—were it not his theory but that of some one else—we might reply that he has used it when endeavoring to account for the frequent occurrence of the *rale* in non-malarial

cases, ("without impeaching the testimony of those who have sought to affix" that *rale* "to the first stage of pneumonia as the diagnostic sound,") and that by thus pressing this theory into service when the exigencies of his position required it, he has fully endorsed, and become as much responsible for it, as if he were its original author.

In speaking of the hypothesis advanced in explanation of the mechanism of the crepitant rhonchus, viz: that it is caused by the bursting of minute bubbles in the air-cells and terminal tubes, it is remarked by Walshe that, "if this theory be well founded, there is no *à priori* reason why crepitant rhonchus should not occur in any pulmonary affection, where fluid may accumulate in the vesicles. But as matter of *clinical experience*, the combination of rhonchal characters described is *almost peculiar* to the early stage of *pneumonia;* whether this depends on any special qualities of the fluid in that disease, or on the *rarity* with which the actual air-cells contain fluid in any other affections, is yet open to inquiry." (*a*)

3. But whichever of the opposing and inconsistent theories adduced by Dr. Manson be adopted—whether the fluid assumed by both to give rise to the crepitant rhonchus, be the result of inflammation, or of simple non-inflammatory congestion, or of any other affection capable of producing "*a viscid fluid in minute tubes*" —the *rale* is stated by Dr. Manson to be seated in "*the air-cells and minute bronchi*"—"*minute tubes.*" When this *rale* is heard, then, it must indicate the precise seat of the disease in which it occurs; and the extent of surface over which it could be heard, would denote the extent of that disease, unless the disease were too far removed from the surface to be recognized by the ear, when, of course, the *rale* could not be heard at all. If this extent of surface were great, it would indicate a grave disease. The reader may remember that, in the second sentence of the section on the "Physical Signs, &c.," Dr. Manson denies that by the crepitant rhonchus "alone," either "the extent, precise seat, or grav-

(*a*) Op. Cib., 114.

ity of the disease" (pneumonia) can be "estimated." We would here remind him of the teaching of his favorite, Skoda, who declares that "fine, equal-bubbling *rales*, if clear, *must of necessity arise immediately beneath the spot where they are observed.*" (*a*) Suppose, then, that Dr. Manson could hear a well marked crepitant rhonchus all over the entire right lung of a pneumonic patient, would he not think that both the "seat" and "extent" of the pneumonia were indicated; and that he had a very grave case before him? It is undoubtedly true that the "gravity" of a disease does not always depend upon its extent; and, so far as we can learn, nobody has ever claimed for the crepitant rhonchus that it "*alone*" denoted the "*gravity*" of a pneumonia; but it is equally true that diseases are usually dangerous in proportion to their extent, although it does not require a large experience to become convinced that certain constitutions speedily succumb to an amount of disease of comparatively trifling extent, while others would withstand the shock of a much more severe attack. In no affection is this well known fact better established than in pneumonia.

"By reason of unaccountable idiosyncrasy," says Andral, "simple engorgement of the lung, occupying but a small portion of this viscus, is sometimes fatal, whilst in another individual, placed in the same circumstances, a red hepatization, occupying more space, terminates in resolution. Such cases are luckily very rare, and do not destroy the general rules laid down." (*b*)

VIII. In speaking of the viscid pathognomonic secretion, (the result of *inflammation*,) through which the air passes in producing (according to him) the crepitant rhonchus, Dr. Manson proceeds to allege that:

"Although this fluid, as proven by its expectoration, is present in all varieties of pneumonia, yet it is usually more fluid and more sanguineous in this than in simple pneumonia, and we should, therefore, rationally conclude that the passage of air through liquids of varying tenacity, density and composition, would produce sounds of a variable character."

(*a*) Op. Cit., 152.

(*b*) Med. Clin. Vol. II, 205.

1. Dr. Manson's reasoning under this head involves a statement of fact and a logical deduction. We contest as well the allegation as the inference. What evidence has Dr. Manson furnished that the expectoration in "*malarial* pneumonia" is "usually more fluid and more sanguineous" than it is in other varieties of pneumonia? Not a particle. The statement rests solely upon his simple assertion, and, in our humble opinion, he will not find it easy of conclusive demonstration. Let us suppose, however, by way of argument, that Dr. Manson's postulate be granted. Then, as the expectoration in question is said by him to occur only in the majority of cases of "malarial pneumonia," the crepitant rhonchus could only be different in the majority of those cases, and must be the same, as in simple pneumonia, in those of the former in which the character of the expectoration is admitted to be identical. But why should the alleged character of the expectoraton in the majority of cases of "*malarial* pneumonia," prevent the evolution of the crepitant rhonchus? Whether the expectoration be owing to the admixture of "*blood* and *mucus*," (as stated by Dr. Manson in his description of the Symtomatology of the disease,) or "partakes more of the character of *blood* mingled with *serum*," (as affirmed by him in the section on "Diagnosis,") still, it is described as being somewhat "*viscid*," and, from its constitution, could not be otherwise. Why, then, should not the crepitant rhonchus be elicited, since, according to one of Dr. Manson's theories for explaining the mechanism of this rhonchus, it is "simply the sound produced by the passage of air through *a viscid fluid* in minute tubes"? Indeed, *this* is the theory in which Dr. Manson says he "*believes*."

2. Let us now suppose that Dr. Manson's *inflammatory* theory (as we may designate it for the sake of distinction,) in explanation of the cause of the crepitant rhonchus, be applied to the case in hand. How will the matter stand then? If we apprehend correctly Dr. Manson's meaning in the statement "that the passage of air through liquids of varying tenacity, density and composition, would produce sounds of a variable character," he inculcates the doctrine that the character of a *rale* is determined by the character of the secretion through which the air passes in producing it. In the sentence immediately preceeding, he tells

us that, in simple pneumonia, the crepitant rhonchus "is *justly* and *reasonably* ascribed to the passage of air through the viscid *pathognomonic* secretion in the air-cells and minute bronchi, that secretion being considered the result of *inflammation.*" Well, if this theory be true, if the "*secretion*" be "*pathognomonic,*" is not the *rale* "pathognomonic" also? We are assured that the "secretion" *is* "pathognomonic"; ergo, according to this doctrine, so must be the *rale.* And yet Dr. Manson denies that it is "pathognomonic"! So much for his own reasoning.

But Walshe disposes of Dr. Manson's logic in a very summary manner. After combatting the "notion that the consistence of the secretion occupying the cells exercises material influence upon the rhonchus," he adds: "Both the ordinary true crepitant and the exceptional sub-crepitant rhonchi occurring in pneumonia *co-exist with expectoration of various degrees of liquidness or viscidity.* Upon this point I perfectly agree with M. Grisolle, nor can *I imagine that any one will be found ready to dispute the position.*" (*a*) And if *Dr. Manson* deems *Skoda* entitled to special respect, we will quote *him* as distinctly affirming that we are *not* able, "by the character of the *rale,* to ascertain the nature and quantity of the fluid which occasions it." (*b*)

[*To be continued.*]

SEVEN CASES OF STRANGULATED HERNIA, TREATED BY ROBERT GIBBON, M. D.

of Charlotte, N. C.

THE following briefly reported cases may be of some interest to the Profession, as showing the different modes of treating this frequent and formidable complaint—namely, the knife, opiates and anesthetics!

CASE 1.—Mr. B., aged 60, a man of robust constitution, while ploughing a rough piece of ground was suddenly obliged to stop work from symptoms of strangulated hernia—got to his house with difficulty. I saw him 12 hours after, and upon exami-

(*a*) Phys. Diagnosis of Diseases of the Lungs, 241.
(*b*) Op. Cit., 148.

nation found the case one of Strangulated Inguinal Hernia—the tumour being the size of a hen's egg, very hard, tense and tender upon touch; vomiting had continued from the first with little intermission, and had become stercoraceous. Failing in all my efforts to reduce the parts, after careful and continued application of the usual remedies in similar cases, before resorting to the knife, as taxis, emetics, blood-letting, hot bath, fomentation, cold, &c., I proceeded with the assistance of Dr. J. W. Caldwell, to relieve the strictured parts with the knife—cutting down and dividing the stricture at the external abdominal ring, without opening the sack, reduced the bowel with little difficulty, and closed the wound with three sutures.

Our Patient expressed himself very much relieved with the exception of the vomiting, which continued, and in spite of all my efforts to check it, the poor fellow continued vomiting for the next succeeding ten days, not being able to retain on his stomach anything whatever in the shape of nourishment or physic. During this time I continued the administration of enemas of purgative and chicken-water, but all without any discharge from the bowels. On the eleventh day after the operation, I made an external application of croton oil to the abdomen which in the course of two hours acted upon the bowels seven times—after which the vomiting ceased and the patient recovered with little interruption.

Case 2.—Mr. D., aged 70—Was called to this patient during the night, and found him suffering from Strangulated Scrotal Hernia, the protruding parts or tumour about the size of a quart cup, very hard and tense, but not tender upon touch—had been vomiting some 20 hours. Failing to relieve the strictured parts by the usual remedies before resorting to an operation, upon consultation with Drs. Samuel and J. W. Caldwell it was decided that I should perform the operation by candle-light, rather than run the risk of the bowel being injured by waiting for daylight; the parts being very large and tight, with the stricture evidently at the external abdominal ring. I cut directly down upon the latter, dividing the stricture with an audible snap, whereupon the bowel was returned with little difficulty—the vomiting ceased and our Patient recovered without a bad symptom.

Case 3.—A boy aged 10, was visited during the night and found suffering from a Strangulated Inguinal Hernia, had been vomiting some twelve hours, the tumour about the size of a hen's egg, and excessively painful to the touch, so much so that it was impossible to use any force for the purpose of reduction by taxis. Failing to relieve my Patient by blood-letting, emesis, hot bath, fomentation, cold, clyster, &c., I urged upon the parents of the boy the necessity of an operation, as being the only certain means of avoiding mortification of the strictured parts, to which they would not consent until seeing their family Physician, by which time I felt satisfied the time for an operation would have passed. As a last resort I gave my Patient an old opium and quinia pill every hour—the pill was not vomited, and after the third dose they began to have a very quieting and soothing effect, and in six hours the tumour entirely disappeared and the child soon recovered.

Case 4.—Mr. W. aged 60—Had an urgent call to visit Mr. W., who had been vomiting some hours from a Strangulated Scrotal Hernia, which he had failed to reduce. Not being able to return with the messenger, I gave him a box of old opium and quinia pills, directing him to give two of the pills every hour, and to place a large bran poultice over the tumour as warm as could be borne, promising to see the patient myself in the course of six hours—at which time I found him sleeping quietly, and upon removing the poultice discovered the tumour had disappeared, to the great satisfaction of all, and no one present questioned the powers of the pills.

Case 5.—Mr. H., aged 70—I was called to see Mr. H. in consultation with Dr. T. C. Neal, of this county. Found the patient suffering from a Strangulated Femoral Hernia. Had been vomiting 110 hours, and as Dr. Neal had failed in all his efforts to reduce the tumour, and the patient was evidently giving way from exhaustion, I advised an immediate operation.—The tumour was the size of a guinea-egg, very hard, with little tenderness. I cut down to and opened the sack; found the bowel considerably discoloured and slightly adhesive to the sack, the stricture at Pouparts ligament, in dividing which I cut the epigastric artery, although I took the precaution to draw the cut-

ting edge of the knife over the sole of my boot before using it in dividing the stricture—there was considerable hemorrhage from the divided vessel; reduced the bowel with some difficulty and force; closed the opening with three sutures; vomiting ceased; administered an opiate for the purpose of keeping the bowels quiet; patient expressed himself very comfortable; no fever nor action from the bowels. On the fifth day after the operation gave a small dose of castor oil, which, in the course of a few hours, made its appearance at the wound; the stitches gave way and the cut became a permanent opening for the discharge of fecal matters; no fever or symptoms of peritonitis; little apetite; continued in this condition up to the 17th day, when during the night there was a profuse hemorrhage from the neighborhood of the injured bowel; patient being much prostrated from the loss of blood. Hemorrhage returned on the 21st day and our patient expired on the 22d day after the operation.

Case 6.—Negro Sam, aged 60—Visited Sam in consultation with Dr. J. Gribble, and found him suffering from Strangulated Scrotal Hernia, the tumour the size of a half gallon measure, very tight and hard; had been vomiting some 20 hours. The hernia had not all been reduced for the space of two years, from which fact, and upon examination, I was satisfied that attachments had been formed. I put the patient fully under the influence of a mixture of chloroform and sulphuric ether, and with a pair of plough lines, drew him up to a beam in the kitchen, heels uppermost; and by taxis succeeded in reducing sufficient of the tumour to relieve the strangulation; vomiting ceased and the boy soon got about again, with the tumour very little reduced in size.

Case 7.—Mrs. M., aged 70—Was called in the night to see Mrs. M. in consultation with Dr. J. W. Caldwell, and found the patient suffering from a Strangulated Femoral Hernia, tumour the size of a walnut; had been vomiting some 48 hours. All efforts at reduction had proved in vain. We put her fully under the influence of the mixture of chloroform and ether. Position to relax the muscles; and after long and continued efforts by taxis succeeded in reducing the tumour; vomiting ceased; gave

an opiate to keep the bowels quiet. Patient lay some ten days quite indisposed, but finally recovered.

A CASE OF STONE IN THE BLADDER.

Reported by Chas. E. Johnson M.D., Raleigh, N. C.

Late in the summer, or in the beginning of autumn of the year 1855, Mr. John Devereux, an intelligent planter on the Roanoke, sent up from his plantation a negro man, Scipio, a weaver by trade, to be placed under my care for medical treatment. He had had during the spring and summer, ague and fever and bilious fever. When I first saw this patient, I found him suffering from general bad health, amounting, in its outward appearance, to that condition known as Cachexia Africana. But upon examination, his case presented the following prominent symptoms: obstinate constipation, great enlargement of the spleen, a tumid abdomen, fulness over the region of the liver, a frequent pulse, constant feverishness, unremitting pain in the loins, considerable emaciation, and unmistakable indications of stone in the bladder.

At my next visit I sounded my patient in the presence of his master, and readily detected the stone lying in the fundus of the bladder, and both of us adjudged it to be of considerable size, Mr. Devereux, at my request, having handled the sound as well as myself. It was evident, however, that this man must be relieved from the inactive and enlarged condition of his liver, his obstinate constipation, enlarged spleen, and constant fever, before an operation for the removal of the stone from his bladder could be ventured upon with the hope of success.

For this purpose, I commenced the treatment by giving a moderate dose of calomel at night, followed by a dose of oil the next morning. This prescription produced only three or four moderate discharges from the bowels, which were hard and whitish, excepting the last: that was softish, but of a slaty color. This satisfied me that I had accomplished nothing beyond simply emptying my patient's bowels with the doses of calomel and oil. I therefore determined to wait a day, and then give the calomel and oil as before. This prescription had a better effect on all the

secretions, than the previous one had, especially on those of the liver; but still I had not accomplished with the calomel all I wished, so, two nights afterwards, I gave him the calomel again, followed by the oil next morning, which had a very fine effect indeed.

While this medication was going on, I had the epigastric and right and left hypogastric regions well painted with strong tincture of iodine, until the integrity of the cuticle was evidently destroyed, a burgundy pitch plaster applied over the loins, and my patient kept on a nourishing but very digestible diet. Having discontinued the calomel and oil, after the third dose, I resorted to the following prescription: Blue mass grs. three, Rhubarb grs. two, Aloes and Ipecac *aa* grs., one—to be made into two pills and given to the patient every other night, unless oftener needed by him to ensure a discharge daily from his bowels. The tincture of iodine was re-applied after the removal of the dead cuticle.

The pills of blue mass &c., were continued about a fortnight, and the tincture of iodine re-applied as often as the cuticle would be removed by previous applications, until all traces of enlargement of the liver and spleen had disappeared. Long before this was done, however, and after discontinuing the blue mass &c., I put my patient on the use of the following prescription: Quinine grs. 36, Iodine grs. 6, Ipecac grs. 4, Sulph. of Iron grs. 8, Comp. Ext. Colocynth grs. 12, to be made into 24 pills. Of these pills the patient was ordered to take four a day, one in the morning, one at noon, one at nightfall and another at bed time.

Here I ought to remark that my patient had been gradually improving in health since he took the second dose of calomel and oil, and I now entertained hopes of being able to operate on him soon for the removal of the stone in his bladder. But the improvement in his general health, and, indeed, in all the symptoms of his case, even those indicating stone in the bladder, a short time after he commenced taking these last pills, was so obvious as to attract the attention of his master as well as myself. Still, I never thought of being able to cure this man of stone in the bladder in any other way than by an operation, and at my next visit sounded him more carefully than I had done for some time

past. The evidences, however, of the existence of stone, by sound and touch, were much fainter than they were on former occasions, and, although I continued to use the instruments daily, in the most careful and painstaking manner, the sound and touch grew fainter and fainter until I could not detect the stone at all. The patient now assured me that he felt as though he had got rid of the stone entirely. for he could retain his water as long as he ever could, discharge it as freely, and altogether without pain or straining. All these facts determined me to call Mr. Devereux's attention to the state of the case, and apprise him of my intention to wait the issue of further medication.

An operation having been abandoned, at least for the present, my patient was directed to do odd jobs about the lot, which, it was believed, would give him sufficient exercise to keep all his organs in healthful play, and to continue the use of the pills of quinine, iodine &c. This plan of treatment was pursued for six weeks longer, when, there being no evidences whatever of stone in the bladder, and the man seeming to be perfectly well in every respect, I sent him home, with the assurance to his master that he was cured. This was in the beginning of the year 1856, and ends my first acquaintance with this case, excepting that I learned occasionally from Mr. Devereux, that Scipio continued well and did his work.

In the summer of 1858, this man was again brought to Raleigh, and put under my care. His condition this time was different in some respects from what it was before, although he had had ague and fever, and a slight bilious fever, in the autumn of 1857, to precede the symptoms of stone in the bladder and general bad health, which he suffered from during the succeeding winter and spring. Early in the summer of 1858, the dysentery prevailed on Mr. Devereux's plantation, and Scipio, as might have been expected, was among the first to take the disease. From the constant straining he was almost perpetually subjected to in making water, on account of the stone in his bladder, which was of considerable size, the dysentery proved to be troublesome and of long standing, aggravating the sluggish liver, slightly enlarged spleen, and constant feverishness with which it found him.

Such was the condition of this man when he was sent to me a

second time. The indications of treatment were most clear.—Cure him of the dysentery, relieve him of the sluggish liver, enlarged spleen and constant fever, and then put him to taking the pills of quinine, iodine, &c., which seemed to do him so much good before. This was done; yet it was a long time before the dysenteric symptoms were entirely mastered. But the treatment which seemed to cure him perfectly of the former attack, stone and all, appeared now to be only capable of putting him, so to speak, in good condition. It did not affect in the least the stone in his bladder. After trying it, therefore, until everybody, physician, master and patient, became satisfied that it was not going to cure him, I determined to remove the stone by *crushing*, which I did entirely in seven sittings with Horteloups instrument, each time, excepting the last, catching the stone without difficulty and crushing off considerable portions of it. The last time I had more trouble and a much longer sitting than at any former trial. There were two fragments left in the bladder after the sixth operation, which were slightly too large to pass into the urethra, although they would readily engage in the neck of the bladder, and yet so small as not to be easily caught by the instrument. I did succeed, however, in catching each piece at the seventh sitting, and in crushing them so completely, that the fragments were passed a few hours afterwards without difficulty. This stone was very hard and laminated, and composed almost entirely of uric acid. It was impossible for me to determine the size of the stone, because I had to depend on the patient to collect the crushed particles for me, and he did this very imperfectly. He never saved any but the larger particles, I am satisfied, because he did not save anything after the seventh operation, alleging that it was so fine he could not collect it, and that he had never tried to save "the sand" as he called it. He also stated to me at the same time, that he was in the habit of going out doors, off to himself, to have a stool, and that on such occasions he never attempted to save anything unless it was so large a piece as to cause him much pain and straining to pass it. Under such circumstances, I obtained one ounce and forty-eight grains of the stone.

This man never kept his bed or had a fever after a single one

of these operations, and two weeks after the seventh sitting, was sent to the plantation to all appearances perfectly well.

RALEIGH, N. C.

A CASE OF NYMPHOMANIA.

Reported by R. L. Payne, M. D., of Lexington, N. C.

This distressing case of mental, moral, and physical disease, came under my care not very long since. It presented in itself a most pitiable condition, showing that women of pure, unsullied virtue can, and sometimes do fall, unwillingly too, down to the very lowest, and most loathsome degrees of wantonness. Miss ——— was of a respectable family, and was highly respected herself. She had been receiving the attentions of a young gentleman, to whom she was engaged, and who discontinued his visits suddenly, without giving the cause. Two weeks after his desertion, I was called to see the lady, and found her laboring under great mental depression,—so great indeed, that I was apprehensive of mental derangement, yet I really did not then think to find in her a case of that deplorable malady, nymphomania. She was somewhat emaciated, and suffered from insomnia and anorexia. She insisted upon it, that she had a prolapsed uterus, and was *more* than solicitous that I should examine her per vaginum, which I declined doing for some time, for obvious reasons, however, I did after a time examine her, hoping that I might relieve her fears, and thereby accomplish good. Her womb was in its proper position, but the hymen gone, the only remains of it being the carunculæ myrtiformes. I assured her that she had no prolapsus, upon which she said she had "such a burning." I asked her where? she answered by indicating the clitoris, in the most immodest manner possible. Every time I visited her, she insisted upon an examination being made, and as all my scruples had been removed by the first examination, I did repeat it once or twice, and I remarked that she took very evident delight in them. Her nymphomania became more and more marked, and sometimes she raved with all the fury of madness. She talked almost incessantly about men, and when with them would throw

herself upon them, and seat herself in their laps, but never acted so towards women. I have never seen, and truly hope I never shall see any other of the gentle sex, so lost to all modesty, so lost to all virtue, in fact to all of those intrinsic qualities which render pure women the very loveliest works of God's creation! This wantonness, as I afterwards learned, was the cause of more than one lover's desertion. Her emaciation became greater daily, her pulse being frequent and feeble, and her condition one of general debility. The treatment of the case consisted chiefly in the use of the ferruginous tonics, and in stimulants. Her bowels were opened daily by laxatives; opiates were given to procure rest at night, and daily exercise on horseback, and in other ways calculated to divert her mind to other objects and from herself, was recommended. I also advised a nutritious diet. About the third week of her indisposition, Dr. W. D. Linsay was called in consultation, and we agreed to continue the above treatment, and at the same time try the revulsive, or counter-irritant effect of repeated blistering over the sacrum,—knowing that it would not only act as a counter-irritant, but we hoped, would also serve to concentrate her attention on itself alone, being in a very inconvenient position. She improved daily on this treatment, and is now, I am happy to say, sound physically and mentally, and let us hope that virtue too has once again enshrined itself in her heart of hearts.

VESICO-VAGINAL FISTULA, CURED WITHOUT AN OPERATION.

By Jno. T. P. C. Cohoon, M.D., of Elizabeth City, N. C.

Mrs. G. aged 26, applied to me about three months ago, suffering, as she thought, from Leucorrhœa. She had been married four years, never borne a child, nor had she ever menstruated but *once*, which was shortly after her marriage. Since that time she has had more or less pain about the lumbar region, with considerable fullness and oppression below the pubes. For two years she has had a discharge resembling that of Gonorrhœa in recent cases, being very offensive, in fact so much so, as to render a visit to her bed-chamber very disagreeable. This was attended

with a constant disposition to *urinate*, often not passing more than a few drops. The discharge and flow of urine kept up such a continued irritation, that excoriation of the vulva was inevitable. Notwithstanding this amount of disease, her general health was excellent, appetite very good, and digestion fine. This was her condition when I saw her in January 1859. I must confess her case rather puzzled my diagnostic powers. However, I treated her for the local symptoms, upon general principles, until I almost succeeded in arresting the discharge of purulent matter, but the urine still continued to dribble away, and the frequent micturition was still existing. I now told her a per vaginum examination was her only hope of a *chance* of relief. She had hitherto positively objected, she now consented. I first washed out the vagina, and then made the examination with the speculum, and to my great joy found what solved the whole mystery, and made a *plain case*, viz: a vesico-vaginal fistula. She positively refused to undergo any surgical operation, but insisted that I should continue my attendance until she either died or recovered. I put her under the antiphlogistic treatment for four weeks; this prostrated her considerably, but seemed greatly to benefit the local suffering. I then gave her cholybeats, during which time I used diuretics and a strong solution of nitrate of silver as a wash. I caused the patient to lay on her abdomen nearly all this time.

Three months have now elapsed; the discharge of pus has ceased, the urine passes through the natural channel, she urinates only twice or thrice in 24 hours, and without pain or difficulty. The fistula has *healed entirely*. I have discharged her, and consider her *well*.

Pittsboro, N. C., 1859.

Mr. Editor:

I most sincerely hope that those imbued with a thorough knowledge of our noble, yet ill-requited profession, may be aroused to the importance of having within our State, a well conducted medical journal. But, my dear sir, there is a feebleness, a want of energy, a decided preference to be ignorant of the important

discoveries of the age. It may be true that ignorance (sometimes) is bliss, but I fear North Carolina, in comprehending medicine as a science is (as usual) far behind. That a medical Journal should tend to correct this apathy to a certain extent, will hardly be denied by any reflecting mind; but as long as we have a certain class of *Cross Road Professors* to operate against, progress in this direction will be slow. I have heard graduates crying down the bill for a medical board, asking, "what right have *they* to examine?" and declared it was a reflection upon their alma mater, all of which amounts to a want of confidence in their ability to stand a rigid examination, prefering to pander to the vulgar prejudice against scientific research, instead of assisting in elevating their profession. But, as Mr. Burton would have said, "Sir, enough of such characters, they are a reflection, Sir, upon the doctorate." I trust that *the* bill passed by the legislature during its late sitting, will form the nucleus for a brighter future.

I see that the French are elevating the medical standard, requiring (as formerly) the degree of A. B. before registering at the faculty. Speaking of France, I see it mentioned that Bozeman succeeded in his operation for vesico-vaginal fistula. He operated in the presence of *Robert* and others, upon a patient of theirs whom THEY had failed to relieve. Strange that an American should be in the midst of the medical savants of France, teaching them surgery. Olivier's paper "On the production of bone," read at a late meeting of the Acadèmie des Sciences, will produce quite a sensation among Physiologists, and I hope, assist us in treating ununited fractures, though Professor Brainard's method (subcutaneous perforation of the bone,) has succeeded remarkably well. Have you seen the account of our New York "*swab*" Doctor (Green) and the New York Academy of Medicine? Dr. Beales certainly relieved him of some of his vanity, and will bring the probang into less repute. Our countrymen seized upon the probang treatment, just as the English did upon Graefe's Iridectomy, but alike have been compelled to abandon it. The boy who was struck on the malar bone has recovered, with so little change in the bone, that it can *hardly* be detected by the touch, though a cicatrix is visible, and adheres to the bone.

Have you observed, following a thorough administration of ether, total suppression and retention of urine for several hours, accompanied with a powerful desire to urinate, afterwards a tremendous flow of urine, similar to that thrown off after a severe attack of Hysteria. My attention was called to it some time ago, in an operation where I had to use ether. The patient was very much frightened at his inability to micturate. My reason for asking the question is, I saw in Thompson on strictures of the urethra, that after the ablest hands had failed to pass the catheter, the patient passed his urine freely when *under* the influence of chloroform.

From the locality in which I practice, I fear that nothing worthy of being reported will transpire, and hence a failure on my part to contribute articles, must not be construed into a want of interest in the Journal. I have taken a deep interest in your undertaking, and any service that I can render will be cheerfully performed.

Very respectfully, your obedient friend and servant,

D. S. Waddell.

BIBLIOGRAPHICAL NOTICES.

A Treatise on Venereal Disease—By John Hunter, F. R. S., with copious additions, by Dr. Philip Ricord, Surgeon of the Hôpital du Midi, Paris &c. Translated by Freeman J. Bumstead M.D. *Philadelphia:* Blanchard & Lea, 1859.

This is an improved edition of a work which has long enjoyed a deservedly and extended popularity. To Hunter and Ricord, the Medical Profession owes a debt of gratitude, which must continue and accumulate whilst the terrible malady they have so thoroughly investigated, shall find a victim, or the skill of physicians is taxed in its management. Replete with important principles, rich in valuable details, and embracing all the laws, facts, and theories which modern investigation has given to the world, it presents in itself the most complete work on Venereal in this or any other language. We recommend it to all who wish a thorough, reliable and accurate book on this most important subject.

The Uræmic Convulsions of Pregnancy, Parturition, and Child-bed. By Dr. Carl R. Braun, Professor of Midwifery, Vienna. Translated from the German, with notes; by J. Mathews Duncan, F. R. C. P. E., Lecturer on Midwifery &c. *New York:* S. S. & W. Wood, 1858.

This is an exceedingly interesting little book, the more so because of the originality of the views presented, and the fact that it is but a single chapter from the great work by Dr. Braun. This is a most complete, philosophical, and reliable work on the subject of which it treats, and, though presenting new and startling views of the nature, treatment &c. of convulsions, it has been most favorably received by the Profession of Europe especially, and is destined to meet with the same reception from American Physicians. We are under obligations to its publishers for many favors.

The Hymen. An Essay delivered before the New York Medical Union. By T. Gaillard Thomas, M.D.

The author of this essay is one of the most talented young physicians in the country, and is destined to reflect great honor on himself and his vocation. Having given special attention to this and kindred subjects, he treats of them *con amore*, and of course, with a proportionate success. We wish him all possible success.

A Treatise on Venereal Disease. By A. Vidal, (De Cassis) Surgeon of the Venereal Hospital at Paris &c., with colored plates. Translated with annotations by George C. Blackman M.D. Prof. of Surgery in the Medical College of Ohio, &c. Third Edition. *New York:* S. S. & W. Wood, 1859.

The reputation both of author and translator must secure a wide circulation for this book. This is the third edition of Vidal's treatise on Venereal, revised from the last French edition issued by the author, and so far as his own views are concerned, must be final. We will give it a more thorough notice at another time.

Science and Success. A Valedictory Address, delivered before the Medical Graduates of Harvard University, at the College in Boston, Wednesday March 9th, 1859. By Henry G. Bigelow M.D.

A chaste, elegant and practical address, worthy of its author, and well calculated to benefit the Profession.

PRACTICE.

OBSERVATIONS ON THE ANATOMY, PATHOLOGY, AND TREATMENT OF THE SPLEEN.

By Greenville Dowell, M. D.

ANATOMY OF THE SPLEEN.

THE spleen is a ductless gland, situated and lying in the left hypochondriac region. It is an oblong, flattened body, of dark bluish red color, about five inches in length, and weighs about six ounces in its natural and unexcited state; but varies a good deal in size during digestion, even in perfect health. It is spongy and vascular in texture. The external surface is convex, the internal slightly concave. It is indented along its middle line and pierced by numerous vessels, which form what is called its *hilum liens*, being irregular openings to admit its large vessels. Its posterior aspect is obtuse, its anterior sharp and notched. The upper extremity is larger than the lower, and somewhat round; the inferior is flattened. By its external or convex surface, it is in relation with the diaphragm, which lies over it and separates it from the ninth, tenth and eleventh ribs; by its concave surface with the greater end of the stomach, the external extremity of the pancreas, the gastro-splenic omentum and its vessels, the left kidney and supra-renal capsule, and the left crus of the diaphragm; by its upper end with the diaphragm, and sometimes touching the left lobe of the liver, and by its lower end with the transverse arch of the color on its left extremity. It is connected with the stomach by the gastro-splenic omentum, and to the diaphragm by a fold of the peritoneum, called the *suspensory ligament*.

It is said by Gross and others, that there have been found from one to eight or ten small spleens, about the size of a hazel nut, and from that to a walnut, connected with the spleen proper by the splenic artery to which they are directly attached. They

are called by anatomists *lions succenturiati*. They are generally found near the greater end of the stomach. I have never seen any of these bodies myself.

The spleen is invested by the peritoneum and an elastic membrane peculiar to itself, which enables it to yield to greater or less extension by its blood vessels. This elastic tissue forms sheaths for all its vessels in their various ramifications. From these sheaths small fibrous bands are given off in all directions. These fibrous bands, called trabeculæ, by their connections, form its areolar tissue. That portion included by these sheaths is soft, granular, of a bright red color, and interspersed with malpighian bodies, small white bodies or corpuscles. These corpuscles are believed to be aggregations of cytoblasts, inclosed in a kind of capsule of capillary vessels. Separate cytoblasts are abundantly scattered through the red substance.

The spleen receives its blood from the cœliac axis by the splenic artery, which is of a very large size in proportion to the spleen. The branches of the splenic artery are distributed to different sections of the spleen, and anastamose very sparingly with each other. The veins are very large, and by their various ramifications compose its principal bulk. The ramifications pour their contents into the splenic vein and help to form the *great portal vein*, which finally passes through the liver. The lymphatics are numerous and of large size; they terminate in the lumbar glands. The nerves are the splenic plexus derived from the solar plexus.

Physiology of the Spleen.

The physiology of the spleen is not well understood, but from late investigations we begin to have more definite ideas of its uses and office in the human economy.

Like the other ductless glands, it does not seem to be essential to life. It may be removed or become perfectly atrophied without the patient's suffering material inconvenience. But in all such cases the lymphatics are much enlarged, and seem to perform its office in the economy of nature.

It is now known that the spleen does modify the blood that passes through it, but how the gland acts to produce this modifi-

cation seems to be a matter of doubt. I believe the blood receives and parts with some of its principles as it does in its passage through the lungs; for we know that the red corpuscles are diminished, and that worn out corpuscles are disintegrated, and in the colored portion of the spleen pulp, corpuscles are found in various stages of degeneration, and these corpuscles help to give it its dark color.

Kirke says (in his Physiology), that "the large size the organ obtains towards the termination of the digestive process, and the increase in the fine granular, albuminous plasma within its parenchyma, and the decrease of these materials, appear to show us that the gland is concerned in elaborating the albuminous or formative material, and for a time storing them up to be gradually introduced into the blood according to the demands of the system." He also observes, that owing to the small amount of fatty matter, it is not supposed that it aids in the preparation of materials for the respiratory process. But it is quite probable that it, like the lymphatics, assists in forming germs of subsequent blood-corpuscles, as there is a large amount of white corpuscles found in the splenic vein.

Besides these, its supposed functions, it acts, no doubt, as *a diverticulum to the portal circulation.* It is used to contain portal blood (after digestion) with its nutritive quality received from the food, and it gradually returns it to the liver, where it is further acted on before it passes to the lungs, etc. This is also made probable by its enlarging in affections of the liver and heart, tending to impede its passage through them, and by its decreasing when the portal circulation is depleted by discharges from the bowels and from the effusion of blood into the stomach.

Pathology of the Spleen.

The diseases and morbid anatomy of the spleen are better understood than its physiology. It is subject to all the various analogous and heterologous formations to which other parts of the body are liable. Inflammation, acute and chronic, abscess, gangrene, hypertrophy, atrophy, softening, suppuration, etc. In its hypertrophied state, it is also liable to be *dislocated* or *lacerated.* It is subject to *induration* and *hepatization*, or sometimes to become

fibro-cartilaginous. It is liable to atrophy or even to entire destruction. Of all the *analogous* formations, the spleen is most subject to hydatids in its substance or on its capsule. It is also subject to fatty tumors and calcareous deposits, sometimes in small bodies like pin heads, one case of which I have seen accompanied with miliary tubercles in the lungs—which they much resemble, if they were not identical.

Serous cysts are found in and on the body of the spleen. These cysts are sometimes filled with melicerous, atheromotous, or steatomotous substances.

There is no pathognomonic sign or sympton of these affections. The spleen is always more or less enlarged in these cases, but they cannot, during life, be distinguished from hypertrophy as a consequence of *fever*. Hydatids sometimes become very large and burst into the abdomen and cause death from peritoneal inflammation.

Of the *heterologous* formations, tubercles are the most common, and will be most generally found where there are tubercles in the lungs or mesentery. An interesting case of this kind I saw in a subject I assisted to dissect at the Louisville University. In this case, the mesentery was completely studded with tubercles, so much so that Dr. Bayless kept the specimen, and it is now in the museum of that college. Melanosis is a rare disease of this organ, but all kinds of cancers are said to occasionally occur in it. None of these affections can be recognised during life. They may be inferred to some extent from the chachectic state of the system and from being known to be present in other parts of the body.

Metastatic abscess of the spleen from wounds and surgical operations are said to be common, but not so often as in the lungs, liver, or brain.

Much the most common affection of the spleen is an acute inflammation of its parenchyma as an accompaniment or sequel to *fever*, disenteries and diarrhœa. Acute inflammation of the spleen, if not promptly and judiciously treated, will most generally terminate in chronic inflammatory fibro-cartilaginous induration, suppuration, gangrene or apoplexy. Inflammation of the spleen is a common affection where I have practised. In the Mississippi and Brazos bottoms scarcely any one lives from three to five

years without having been subject to this affection. I presume I have seen as much of this disease as any man of my professional age, and can speak positively on this subject. For the last twenty years I have lived either on the bank of the Mississippi or Brazos, or near their immediate vallies. Fourteen years on the Mississippi and six on the Brazos; twelve years in active practice, and eight years under my brother, then in full practice at Raleigh, Tennessee. Inflammation of the spleen, as I said before, is common to every one living three or five years in these bottoms. Whites and mulattoes are much more liable to it than pure blacks. In blacks I have never seen a case of enlargement of the spleen except as a complication of typhoid, or rather as a concomitant of typhoid fevers. Griffs are not as liable to it as mulattoes, nor mulattoes as whites. Blacks are partially exempt from intermittent fever, but are equally, I believe, liable to inflammation of the liver and remittent fever. As to affections of the liver, I have not noted any difference in persons of different colors; nor have I discovered any difference as it respects typhoid fever. I have thought, as a general thing, that blacks are more liable to typhoid fever than mulattoes or whites, but I attributed this to their want of cleanliness, which is observable to any one who is familiar with large negro plantations. I have found the spleen partially enlarged and rather indurated in all cases of typhoid fever, which I have examined after death even in a child two years old.

Acute inflammation, as I said before, is liable to terminate in chronic, and from chronic inflammation, we have hypertrophy as a most usual result, and from enlargement we have dropsy, a general anæmic state of the system, and an impoverished state of the blood.

If the active inflammation is not subdued or modified by treatment, the spleen will soften, suppurate, or terminate in gangrene. If the inflammation alone is attended to, and the patient is not treated for the primary cause, say malarial or typhoid fever, the spleen will become gradually enlarged, and often obtains an enormous size; many hundred of such cases have come under my observation.

Hypertrophy may come on gradually and even imperceptibly, but may be easily detected by pressure over the region of the spleen. There is always more or less tenderness on pressure, with flatness on percussion, even before the spleen can be felt. After it becomes large its outlines are easily traced, and from the small size we have described it in health, it enlarges until it covers the anterior cavity of the abdomen, even feeling as if the whole cavity were filled with a solid body. It now causes a protrusion of the abdomen similar to that of a pregnant woman in the ninth month. That the spleen is enlarged may be told also by the general appearance of the countenance and skin. The skin is of a sallow-white color, and may be easily recognised by those familiar with the disease. It may, nine times out of ten, be told by the general appearance of the skin alone, without any tactic examination. In cases of enlarged spleen caused by malarial fever, I have not so generally found the liver implicated, as we are led to suppose from reading the books; it is often found in almost a healthy condition, sometimes, I may say often, secreting bile too freely. Where the liver is not implicated but acting, I have not found mercury necessary to a cure of the spleen, while in those cases where the liver is diseased also, mercury is almost, if not absolutely necessary to perfect a cure. In intermittent and remittent paroxysms I have found a great tendency to *rigors*, if the spleen be enlarged, or enlarges during the paroxysm; but where the spleen was not enlarged, or did not enlarge during the paroxysm, the chill and fever generally came on together. Where the teeth are made to chatter, and there is a general shaking of the muscles, the spleen is always enlarged. this may be called a pathognomonic sign, for I do not remember a single exception. While the spleen is moderately enlarged, and before adhesions have taken place to the surrounding textures or organs, the spleen may be easily *dislocated*, several cases of which I have seen: the suspensory ligament is torn, and the spleen turns over, as it were, and falls forward on the colon and bladder, and here becomes fixed by adhesions, so that it cannot be replaced; yet it may be partially reduced, and the patient may live and even enjoy a moderate portion of health, but always have the sallow appearance which I have described. A case of this kind is now in Brazoria county, in a lad of fourteen years.

Suppuration of the spleen is rare in proportion to the number of cases of enlargement. By some, as the late Prof. Drake, it is supposed never to occur below thirty-five degrees of latitude. This is undoubtedly an error, for I have seen three cases in my county, south of Galveston. A Mr. Bradley had his spleen to suppurate in 1853, and it pointed through the abdominal wall below the ribs, and was punctured by my friend, Dr. F. M. Oatis, of Lordy Point, Texas. It discharged a large amount of dark grumous pus and blood. The orifice healed up; but he still has an indurated spleen of an enormous size, covering most of the abdominal wall. He is at times dropsical, and has a running ulcer on his leg, as a result of the chachectic state of his system. He is able to ride about, and otherwise enjoys good health.

Case 2. Mrs. P., of Brazoria county, was taken with intermittent fever in the spring of last year, 1858, and it continued to occur during the early part of the summer, until finally the spleen suppurated and discharged its contents into the stomach. She was treated by myself and Dr. Porter of Columbia, with mercury, iodine, blisters, and quinine, or the citrate of quinine and iron, but nothing we could do would stop suppuration. There was a circumstance in Mrs. P.'s case worthy of further note: she was salivated with iodine used as an embrocation over the spleen while there was no smell of mercury, or any ulcerations of the gums. The saliva poured out of the mouth, until she thought she would suffocate from it. By discontinuing the use of the iodine the salivation soon subsided, though the mercury was continued more or less for two weeks afterwards, and without any symptoms of ptyalism. She suffered greatly from retchings and sick stomach. The alvine discharges were frequent and of a flux-like character. She finally recovered, but her spleen is still very much enlarged.

Case 3. Mr. Henry, a carpenter of Columbia, had had chills for nearly two years, at irregular periods, and finally was taken so bad that he could not work. He paid a visit to the Gulf of Mexico, for the purpose of trying sea-bathing, and while there his chills got worse on him, and he was brought home on a bed. I was called to see him, and used the usual remedies with energy, but could make no change for the better. I broke up his

fever and stopped his regular paroxysms, but still he did not improve. I blistered over his spleen, which was much enlarged, but could not reduce it. About the fifth day of my attendance he had a hard shake, and thought he was dying. His chill lasted about one hour and a half, and went of with a profuse sweat. It came on him when he had about twenty grains of quinine in his system, which in most cases is sufficient to stop any chill: seeing this I immediately began to suspect suppuration, and I examined for a softening, but could not detect any. His liver was acting well; his stools were bilious; but I thought I could feel the liver lobulated, and it was tender on pressure. He had slight fever after his shake, with a hectic flush on his cheeks. These shakes continued at irregular periods for three days, in spite of all I could do, and he finally died delirious. There was no post-mortem, which I regret; but from all his symptoms, I thought there were several abcesses of small size. His symptoms resembled cases of suppuration of the mammary gland, etc.

Hiccough.

I have seen severe and continuous hiccough from an enlarged spleen. One case, I well remember, occurred to me while practising in Panola County, Mississippi. I was treating a young man for intermittent fever, attended with vomitings and sick stomach. His spleen was enlarged. I gave him some pills composed of blue mass and rhubarb, and followed it with large doses of quinine—ten grains every two hours until three doses were taken before his chill time. This broke his fevers and stopped his chills, and greatly reduced his spleen. But as the spleen contracted, he was taken with hiccough, and it was more distressing than his former situation. I gave him all the antispasmodics I could think of—morphine, camphor, assafœtida, valerian, and warm baths; applied mustard to the stomach, etc., yet in spite of all this, his hiccough continued even during sleep for eight days; at last, not knowing what to do, I salivated him, and as soon as the mercury took effect he began to improve, and with the salivation, he recovered entirely, and has never, as I know of, been subject to this symptom since. I thought in that case, the spleen had contracted adhesions to the diaphragm, and the medicines I

gave him to cure his fever and ague, had cured his spleen and torn up the adhesions, causing the diaphragm to take a convulsive action. The calomel subdued the inflammation of the spleen and diaphragm, and thereby cured the hiccough.

Gangrene of the spleen I have never seen. Softening is a frequent accompaniment of hypertrophy. Atrophy I have never seen, and think it very rare. Fibro-cartilaginous and indurated spleens are common. Where an enlarged spleen is reduced by medical treatment, it almost invariably is left indurated, and its coats become fibro-cartilaginous, and its trabeculæ are enlarged and thickened. The spleen sometimes seems to lose its function, and is not easily enlarged by fevers afterwards. It is not an unusual occurence for a boy, or child, or even for young men, to have enormously enlarged spleens, and to be sent out of the bottom lands to school, to recover entirely their health, and on their return they are not subject to malarial fevers, and especially are they exempt from enlarged spleens. But in these indurated cases the spleen is never found in its natural state or size in healthy persons who have never suffered from enlargement. It never returns to its normal size, and can generally be felt in its place. The spleen in such persons is found after death to be fibro-cartilaginous. The spleen in its enlarged state is often ruptured or lacerated by external violence, by blows with the fist, falls from horses, etc. Several cases of death from such causes I have seen. A case of this kind is reported in the New Orleans Medical and Surgical Journal, by Dr. Chinn, of Brazoria, where an individual died from a blow over the spleen, in a fight. The homicide was tried for murder, but upon a post-mortem examination made by Dr. Chinn, the spleen was found softened and easily lacerated, and upon a statement of these facts the man was acquitted. I also knew a young man to be thrown from his horse, and to die in a few days from pain in the region of the spleen, caused, no doubt, from laceration.

Therapeutics of the Spleen.

The means of subduing inflammation are as applicable to the spleen as any other organ, and should be resorted to immediately, as in all cases of inflammation. Where it is brought on with

or by malarial fever, you must give in addition to mercury, quinine in large doses. Experience has taught us that the spleen can be reduced sooner by quinine in large doses given at once, than by small doses often repeated. Thirty grains given in three doses of ten grains each, are more efficient than thirty grains given in ten doses in twenty-four hours. If there be tenderness over the spleen after you have given an active dose of mercury and quinine in free doses, it is well to apply a blister, which will remove the tenderness and reduce the spleen, if enlarged.

In chronic inflammation of the spleen, with enlargement, I give but little mercury, and that generally combined with taraxacum. Blue mass, often repeated, I have found injurious. It is too slow in its action, and creates an irritation of the stomach; but if it be combined with some active purgative, as rhubarb or taraxacum, it answers every purpose and does well. I usually give one of the following pills, but much prefer the first where there is fever:

(1.) Calomel.....x. grs. Taraxacum.....v. " Rhubarb.....vi. " Make into four pills; to be given at one dose.	(2.) Blue Mass.....xx. grs. or Rhubarb...vi. " Make into four pills; to be given at one dose.

If there be no fever, divide this into two doses, and give two pills every four hours. If there be diarrhœa with fever, combine an opiate with the pills, morphia or opium, the latter is best. If those pills do not operate in ten hours, give a dose of castor oil; if they act too freely their action must be checked by opium: it is not well to let the bowels run off, as we say, in any case. After the action of these pills, or if the case be urgent, begin with the following pill: citrate of quinine and iron xxx. grs., extract of gentian or liquorice, sufficient to make into mass; divide into six pills; of these, give one every hour until all be taken, beginning in time to get them all taken before the expected chill. If the patient is disposed to be dropsical, I combine also with this pill some tonic. Oil of black pepper is the best, half a drop to each pill. This should be repeated from day to day, until the enlargement of the spleen cannot be felt, or until all signs of chills or fever are gone. The quinine and iron should be taken

every cold or wet day, until all appearance of the disease is removed. There is a great tendency to relapse in these cases, which should be strictly guarded against. I have tried iodine in every form and every mode I could think of, but I cannot say it ever did any good in my hands; nor have I ever seen it do any good when applied to enlarged tonsils; the functions of the absorbents, or rather the lymphatics, are destroyed by the enlarged spleen, and we must therefore endeavor to cure by endosmosis instead of by intestinal absorption, hence, the great good which blisters do in this disease. Blisters should be small, and be often repeated. I have now abandoned the iodine treatment, and find that I succeed much better without it. I never fail by blisters and citrate of quinine and iron. This preparation is the best of all the various compounds of quinine, in cases of enlarged spleen, or in anæmic conditions of the system. The bowels must, in all cases, be actively moved at least once a day, especially where there is a tendency to dropsy. In dropsical subjects, after giving the above pills, I often follow them with cream of tartar for several days or weeks. In cases of congestive fever where the spleen is enlarged, there will be no serious congestion of the liver or brain, but great congestion of the stomach and in the mesentery is evinced by continual vomitings, retching and diarrhœa, even almost like cholera. There will be great pallor of the surface, and the skin will assume its peculiar shrunken appearance, called cutis anserina. These cases require stimulants of the most active kind—brandy, ammonia, sinapisms, with an opiate. But they will not bear much mercury without producing ptyalism, and it is not so necessary to a cure; but a moderate dose at first I have found generally necessary or beneficial. Our main reliance is upon quinine. It is in all cases a *sine qua non*. Where there is no enlargement of the spleen, the liver or brain is sure to be seriously affected, and then mercury is our *sheet anchor*, and it must not be spared until you overcome the congestion, or affect the system with it. In these cases I give also the *sulphate of quinine*, for that seems to be the most efficient in all cases where the liver or brain is affected.

The best time to reduce an enlarged spleen in the bottom lands is in the fall, after frost, and by continuing your treatment

through winter you can perfect a cure; at any other time relapses are sure to come, unless the patient is very prudent, and often no prudence will avail. Then the patient must leave the bottom lands, or take medicine all the time until the habit is broken up, and the spleen becomes quiet, as it were. I find irritation with Croton oil and sweet oil, xx. drops of Croton to the ounce of sweet oil, rubbed over the region of the spleen, or tartar emetic ointment, to answer a good purpose, where the patient is unwilling to blister. But I would never rely on iodine, as I said before, for I have found it inefficient. This seems strange, from its known and undoubted good effects in bronchocele and in enlargement of the lymphatic glands, but we must remember that the lymphatics are, as it were, destroyed, and therefore cannot act. The red globules must be increased, and also the fibrin, hence the good preparations of iron do in these cases. Unless the blood is enriched by iron, or some tonic, quinine will not stop a long-standing case of chills. It will always stop or check them for a while, but they will return. Portwine is an excellent tonic in these cases. Gin is even better where there is a tendency to dropsy. The Russian belt worn tightly around the body is good in all cases of enlarged spleen, whether it is from hydatids or fever, and where the spleen is dislocated, or the patient is dropsical. Any belt made of leather or cloth will answer, but the Morroco belt made wide is the best. It should be worn tight, and be kept so until the spleen is reduced. I have often cured cases by the following pill, which will be more readily taken by your patient than almost any other combination:

Sulphate of quinine..... ℈ i.
Citrate of quinine and iron..... ʒ ss.
Extract of Gentian..... grs. x.
Hyoscyamus..... grs. xii.

Make into twelve pills: give one every hour until six are taken. This pill, with an occasional dose of the calomel or blue mass pill mentioned before, will cure any case of enlarged spleen or chills, if persevered in a sufficient length of time.—*New Orleans Med. & Sur. Jour.*

SURGERY.

ON THE ARTIFICIAL PRODUCTION OF BONE.

By Dr. Leopold Ollier.

I. The reproduction of bone proceeds from the inner surface of the transplanted periosteum. Wherever a portion of this membrane can be transplanted, new bone is formed, either adherent to the bone from which it was taken, or entirely independent of it, according as it is suffered to remain, partially connected with the rest of the periosteum, or is completely detached from it.

II. In transplanting portions of the periosteum, bone of various forms and dimensions can be obtained, circular, in the form of the figure 8, spiral, etc., according to the shape and position of the flap, consequently, we have perfect control over the direction and extent of the process of ossification.

III. Bones thus developed are not simple, shapeless concretions of calcareous matter; they consist of true bone, found with all the anatomical characteristics of true bone. In proportion to their developement, they resemble normal bony tissue, with an external compact layer, and a medullary cavity in the interior. This cavity is filled with a marrow, resembling, in all respects, that of healthy bones.

IV. The new bone is developed in the sub-periosteal blastema, which exists normally upon the inner surface of the periosteum. This proposition is demonstrated by an examination of the formation of the new bone, and by removing this layer of blastema, when the reproduction of bone is suppressed, or at least, for a time, arrested.

V. This blastema consists especially of free nucleoli, enclosed in cells, floating in a semi-liquid, transparent, or finely granular material, and mingled with more or less fibrinous elements.

These various embryonic elements develop and multiply in the exudation, (at first amosphous) furnished by the capillaries of the periosteum.

VI. The sub-periosteal product, which is observed the first few days following the transplantation, is generally cartilaginous;

but the succeeding development of bone progresses without this intermediate element. This cartilage, moreover, differs in structure and appearance of its anatomical elements, from the normal cartilage of the epiphysis.

VII. An analagous membrane is found after a time, upon the surface of bone, from which the periosteum has been removed.

VIII. When we removed a bone, or a portion of a bone, leaving its periosteum attached to the tissues which normally cover it, we observed at the end of a certain time, that this bone or portion of bone, is reproduced to a quarter or less extent The reproduction is sometimes perfect. The soft tissues cannot supply the place of the periosteum; nor do they directly contribute to the ossification.

The reproduction of bone is in proportion to the amount of periosteum preserved in the wound. When a segment of bone, embracing its whole thickness, is removed, its reproduction depends solely upon the periosteum.

IX. After the resection of the articular extremities of two contiguous bones, a new articulation is formed, if the capsule and ligaments are left on all sides, connected with the periosteum of the resected bones. The two bony extremities are remodelled independently.

The surgical deductions, which we may draw from our experiments, are numerous; but we will only mention the most important, which refer to resections, and to autoplastic operations.

In resections, art should not be content simply to remove the diseased portions; it should also endeavor to reproduce the fragments of bone which it has destroyed. Experimental physiology suggests to us the means; it teaches us why resections have hitherto been so seldom followed with reproduction of bone, and shows what course we should follow in the future. The preservation of the periosteum is of the highest importance. We have endeavored to demonstrate in a recent work, that, in spite of certain inherent anatomical and physiological difficulties, this is practicable, partially at least; and we have proved that clinical observation has already fully confirmed the truth of the assertion. As regards autoplasty, we think that the continuation of ossifying

secretions from the wide surface of the transplanted periosteum, increases its importance and extends its application. Since, in animals, we obtain bone wherever we transplant periosteum, we can expect similar results, under certain circumstances, in man, by obtaining portions of periosteum within the flaps. Innumerable difficulties arise in our mind, when we reflect upon the realization of this idea, which we should see in order to avoid them. But they do not seem wholly unsurmountable.

Periosteal astroplasty (operations, whose object is to reproduce bone by means of transplanted periosteum) is now a rational undertaking.

There is still another application of these principles not directly illustrated by the experiments just reported, but yet in a measure related to them, which we have verified in especial experiments: we refer to the rational mode of diminishing the chance of suppurative inflammation of the bones, after amputations, and of inducing thereby union of the flaps by first intention. The periosteum unites with bony more readily than with any other tissue. The nature of the organizing elements which cover the inner surface of the membrane, and its especial function in ossification, enable us readily to comprehend the causes of this result, which experiment also demonstrated.

We believe that it will prove very useful after amputation, to cover the end of the bone and medullary canal with a small piece of periosteum.

Experiments upon animals illustrated the feasibility of this plan already proposed, but too often neglected, as it seems to us, without sufficient cause. The reasons which lead us to recommend it now, are not those formerly given; they are based upon physiological facts. The difficulty of the operation ought not to influence us, when so much advantage is at stake. We need fear no accident; at most, this step in the operation may prove simply useless.

ON THE OPERATION FOR HERNIA IN CHILDREN.

By Dr. Ravoth.

(Deutsche Klinik, 1858. No. 29.)

Dr. Ravoth communicates an interesting case of inguinal hernia occurring in a male child, fourteen months old. Observed

soon after birth; a truss had not been applied to it until the child was six months old, and this, owing to the frequent projection of the large hernia, seems to have been ill-fitting. When the child was seen by the author, incarceration of the hernia had taken place during forty-eight hours. The scrotum was distended with a tumour as large as a man's fist, having very much the appearance of a hydrocele. As repeated attempts with the taxis, under chloroform, were found useless, the operation was at once resorted to. In order to avoid loss of blood, two small arteries, which were divided while laying the sac bare, were at once tied. The parts were replaced without opening the sac, and the wound was simply dressed, a little pad of charpie being laid over the inguinal canal. Chloroform was employed during the operation, and frequently since during the dressing of the wound, on account of the child's cries—the hernia then very easily redescending. In about three weeks the wound had cicatrised, and a properly fitting truss was after a while well borne.

In reference to the history of this case, the question may be asked, when is the most suitable time for the application of a truss in infants? Most practitioners say not before the sixth month, and some even not before the twelfth. This advice is grounded upon the great sensibility of the child's skin, the necessity of great cleanliness, the rapid growth of the child rendering a frequent change of the truss necessary, the rarity of strangulation in childhood, and the frequency with which a spontaneous cure is brought about in congenital hernia. Although these reasons may have their weight in particular cases, the author does not consider that they have validity enough to raise this practice into a general rule. On the contrary, he believes that the truss should be applied at the earliest possible period, and chiefly because the partial or total protrusion of the hernia gives rise to much abdominal pain and gastric disturbances, causing crying and restlessness, and impeding development. The hernia, too, increases in size, and the abdominal ring in width, so that the tumour is retained with more and more difficulty, and a radical cure, which at this early age may usually be expected from a well-fitted truss, becomes more unlikely to happen. The pressure from the large pad and strong spring, too, now required, will not

be borne by the child's delicate skin. Finally, there is the danger of strangulation, which at this early age is not always easily detected, and the operation for the relief of which is one of great danger.

The successful issue of the present case is chiefly attributed by Dr. Ravoth to three circumstances—viz., the promptitude with which the operation was performed; the executing this without opening the sac, and dilating the ring with a blunt hook in place of a cutting instrument. This child had rejected all food during twelve hours, and had been deprived of refreshing sleep during forty-eight hours; and had a further delay taken place from the use of baths, clysters, &c., the issue would probably have been different. Moreover, the hernia had not suffered from excessive employment of the taxis, and chloroform was resorted to during its application. After repeating the well-known arguments in favour of not opening the sac, Dr. Ravoth observes that in children this should never be opened when there is any possibility of avoiding it, and especially in the case of large hernias, where repeated protrusion may be produced by the child's cries and movements before the healing is accomplished. In the present instance the hernia was very large, containing the cæcum and *processus vermiformis*. The old practice, now almost forgotten, of attempting to dilate the aperture without the use of a cutting instrument, should, when practicable, be preferred. As far as the author is aware, there are about thirty cases on record in which the operation for hernia has been performed on infants and young children; and of the seventeen operations of which the issue is given, nine terminated in death.—*Med. Clin. Review.*

MIDWIFERY.

ON HYPERTROPHIC ELONGATION OF THE NECK OF THE UTERUS IN PROLAPSUS UTERI, IMPROPERLY SO CALLED, AND ON ITS TREATMENT BY AMPUTATION OF THE NECK OF THE UTERUS.

By M. Huguier, Surgeon to the Hopital Beaujon, Paris.

In this remarkable *memoire*, M. Huguier maintains that the ordinarily received opinion is erroneous, viz., that the appear-

ance of the os uteri at the vulva, or of a greater or less portion of the uterus itself beyond the external organs of generation, is the result of a displacement of the organ, of a general lowering, or of a true prolapsus *en totalité*: and he holds that this opinion can be shown to depend on inaccurate observation. When the uterus appears externally, and even when the vagina is completely inverted, and the uterus from the size of the tumour in the centre of which it lies, appears to be entirely prolapsed between the thighs, it is not because it has become generally lower in level, and completely left the pelvic cavity, but simply because it has undergone a partial or general (as the case may be) hypertrophic elongation. The best proof that can be given of this is, that in almost all the cases, the body of the uterus remains nearly in its usual situation; and that, if we examine the tumour with care, and measure the uterine cavity by means of the uterine sound, or any other approriate instrument, the elongation is easily detected. And we can equally assure ourselves of the presence of the body of the organ in the pelvis by careful palpation of the tumour, and by the introduction of one or two fingers into the rectum. M. Huguier does not treat of all the various kinds of uterine hypertrophy; but confines himself to the consideration of the longitudinal hypertrophy which simulates, accompanies, or determines the descent of the uterus. The two principal varieties of the longitudinal hypertrophy—viz., as it affects the uterine neck *above* or *below* the insertion of the vagina—constitute two different diseases, although their nature is the same. Their causes, their mechanism, their symptoms, the accidents they may determine, and even the treatment they demand are, however, quite different. Hence these two conditions demand separate consideration. And 1st, with regard to the hypertrophic elongation of that portion of the uterine neck *below* the insertion of the vagina. This is not of so much importance as the other form of hypertrophy; and we give merely the *conclusions* with which M. Huguier terminates this, the first part of his paper. 1. In this variety, the body of the uterus forms in the cavity of the vagina a cylindrical *swelling*, either resembling a more or less elongated cone, the free extremity of which approaches the vulva, or even passes between its labia without any shortening or in-

version of the vulvo-uterine canal. 2. This elongation, which has been noticed by Morgagni, Saviard, Bichat, Lallemand, Desormeaux, Lisfranc, and Boivin, was considered by them as a simple anatomical variety. 3. Even until lately, it has been commonly confounded with the sinking and descent of the uterus, when it has been mistaken and treated for a polypus, a chronic retroversion, a follicular cyst, a cancerous or a dropsical enlargement of the neck. 4. No anatomical and nosological description has yet been given of it, although it possesses sufficiently precise characters as regards its causes, development, symptoms, and treatment. 5. The medical appliances, and the different kinds of cauterisation, are only applicable to cases of slight hypertrophic elongation, and particularly to those which are of little extent, and are complicated with inflammation and engorgement. 6. When a hypertrophic elongation of the vaginal portion of the os uteri causes serious symptoms, and has attained a length of from five to seven centimetres, there is only one truly efficacious means of cure, viz., the resection of the uterine neck to half a centimetre below the insertion of the vagina. The second part of the paper is taken up with the consideration of the hypertrophic elongation of that portion of the uterine neck situated *above* the reflexion of the vagina, and its escape outside the vulva. M. Huguier affirms that the affection designated by authors and practitioners under the names of prolapsus and complete descent of the uterus is nothing else, in the very great majority of cases, than a longitudinal hypertrophy of the uterus, the body of which remains in the pelvic cavity, even although the vagina, be entirely inverted, and the tumour hanging between the thighs be equal or even superior in length to the uterus in its normal condition. This proposition, startling though it appears, is the result of fifteen years' labour and conscientious inquiry. He denies the frequency of the complete prolapsus, holds that it is rarely seen, and that it has been generally confounded with the hypertrophic elongation of the upper portion of the uterine neck. Indeed, the swelling, which up to the present time has been described as complete descent or prolapsus, may depend on two different conditions,—the one quite exceptional, viz., the true descent of the uterus with or without longitudinal hypertrophy;

the other much more frequently met with, and forming the subject of the present paper. In establishing his proposition, M. Huguier relies on three kinds of proofs: viz., on cases scattered through the records of science, on pathological anatomy, and on clinical observation. Under the first head, references is made to cases related by Saviard, Morgagni, Dance, and Cloquet. Under the second, the magnificent work of Cruveilhier is appealed to; and M. Huguier asks, if cases of the complete descent of the uterus were as frequent as authors pretend they are, how comes it that Cruveilhier has only been able to give a single representation of the affection in a work in which he has been occupied twelve years, and which he closed with the description of the diseases of the uterus and ovaries? With the exception of two cases, observed by MM. Morel-Lavallée and Blandin, M. Huguier knows of no others where the characters of the affection have been described in such a manner as to leave no doubt as to its exact nature. Of course, in this review, cases accompanied by considerable peritoneal effusion, or with very voluminous ovarian cysts, where all the pelvic organs are apt to be pushed downwards, are excluded. Since the year 1843, when M. Huguier began to use the uterine sound, he has carefully examined sixty-four cases of pretended complete prolapsus; and of this number only two were instances of true and complete prolapsus, unaccompanied by hypertrophic elongation. In a third case, there was at once complete prolapsus, retroflexion, and considerable hypertrophic elongation of that portion of the uterine neck above the insertion of the vagina. In this series of sixty-four cases, M. Huguier has included only those which would be considered, according to the description of authors, as instances of complete prolapsus, in which the length of the tumour was at least equal to that of the uterus in its normal condition (though in most cases it exceeded this,) and where the vagina was completely inverted. All cases of slight descent of the uterus, described by authors under the name of semi-prolapsus or incomplete descent, and even those where the neck and vulvo-uterine canal, partly inverted, projected from two to three centimetres from the vulva, were excluded. After mentioning that for some time past he has been in the practice of expound-

ing these facts at his hospital visit, and that M. Robert, at the Hôpital Beaujon, has verified them in his own practice; M Huguier concludes by stating that a careful examination of the various preparations in the Musée Dupuytren, along with the conservator, M. Houel, only confirms the truth of his observations.—*L'Union Médicale.*

ON THE DIAGNOSIS OF PREGNANCY.

By Professor Hecker.

Professor Hecker, of Marburg, has published the result of extensive investigations into the prognostic value of the penetrability to the finger of the os internum uteri in pregnancy. It is generally known that shortly before the onset of labour the os uteri internum opens. Dr. Hecker has endeavoured to give precision to the indications of this phenomenon. Since 1833, when he succeeded to the charge of the Marburg Lying-in Hospital, records have been kept of the date on which the os internum was ascertained to be open, and of the lapse from that event to labour. He of course points to the difficulty, that the time when the opening of the os internum was *felt* was not necessarily that when the os *first opened;* but he believes that the results of a large number of observations compensate for this source of error, although, since all the error is always on the same side, it is not easy to see how the multiplication of cases can tend to its neutralization.

Dr. Hecker remarks that he has several times observed, in the case of women who had walked a long distance to come to hospital, or who recently had undergone violent exertion, that the inner os uteri was penetrable on admission, but closed again after a few days' rest.

His general results are as follows:

Out of 2593 persons examined between 1833 and 1858, there were 946 in whom the inner os uteri was penetrable to the finger. Of these 946, 723 were pluriparæ and 223 primiparæ. Of the 723 pluriparæ,

366 were delivered within 7 days of ascertained penetrability.
510 " " 14 "

586 were delivered within 21 days of ascertained penetrability.
638 " " 28 "
85 " after 28 "

Of the 223 primiparæ,

138 were delivered within 7 days.
169 " 14 "
189 " 21 "
209 " 28 "
14 went over 28 "

Thus it appears that in primiparæ, penetrability of the inner os uteri was followed by labour in seven days in 62 per cent of the cases, and in pluriparæ in about 50 per cent; that labour followed within fourteen days in 75 per cent of the primiparæ, and in 70.5 per cent of the pluriparæ.—*Med. Clin. Review.*

PLACENTA PREVIA

A. T. Dorkins M.D., thus concludes an article in the Edinburg Journal:

"It is from these considerations that, in the cases alluded to, in which something must always be done not only to check the flooding but to advance the labour, that I would venture to suggest *mechanical expansion of the cervix, by means of a sponge tent specially constructed for the purpose*, as a method of treatment. From the operation of this procedure the following results may reasonably be expected:—

1. It would gradually throw off the placenta, and by putting the fibrous structure of the cervix on the stretch, it would compress the utero-placental vessels. In other words, it would, by its action on the cervix, detach the placenta and arrest the hæmorrhage *pari passu*.

2. It would act both as a plug and as a powerful compress applied to the opened mouths of the utero-placental vessels.

3. It would excite uterine action.

In order to produce these important effects, the *sponge tent* employed would require to be rounded at its upper extremity to prevent its introduction injuring the placenta; it would require to be of large size, and so constructed as to expand rapidly under the influence of tepid injections."

PHYSIOLOGY.

ON THE ALLEGED SUGAR-FORMING FUNCTION OF THE LIVER, AND THE INFLUENCE OF DIET ON THE LIVER.

By F. W. Pavy M.D., London.

This communication is one of exceeding interest and importance, certainly in these respects not second to any in the whole volume. The experiments detailed by Dr. Pavy, and the deductions he draws from them, will aid materially in the solution of the problems that now occupy most the attention of physiologists. From the care and skill with which the experiments were evidently made, and the great number of times they were repeated, we must rely upon the results uniformly obtained by them. The deductions drawn from them are fully warranted, indeed forced upon us. A little reflection, too, will show clearly that they are not only not opposed to the facts obtained by Bernard and others, in their experiments, but entirely in accordance with these facts, and only opposed to the *interpretation* hitherto given to them.

Experiment has proved, beyond a doubt, that a large quantity of sugar is met with in the liver, and in the blood of the hepatic veins, and of the right side of the heart of an animal that has been for some time previously restricted from the ingestion of saccharine materials, *after the death of the animal.* The question Dr. Pavy desires to solve is, whether sugar is natural to the right ventricular blood of the *living animal* and to the *living liver*, for to find sugar on examining the blood or the liver after death is no proof that it was also present during life. The results he has obtained in endeavoring to solve this question are very striking, and lead one irresistibly to the conclusion that during life sugar does not exist in the liver, but a substance which happens to be with extreme facility, by a process allied to fermentation, convertible into sugar, and which has the power to resist transformation into sugar, whilst located in the tissue of the living and healthy liver. With the destruction of life this power of resistance is at an end, and the organ then becomes speedily charged with the saccharine principle.

When blood was removed by Dr. Pavy during life from

the right ventricle by means of a catheter passed into the right jugular vein, it was found to contain only the merest trace of sugar, the reaction with the Barresvil solution being sometimes so slight as to be liable to be overlooked altogether. Dr. Pavy made upwards of sixty observations to establish this fact. When in these cases the animal was killed, the blood collected after death from the right ventricle occasioned with the same reägent an abundant orange-yellow precipitate of suboxide of copper.

When an animal is pithed, and a minute or two is allowed to elapse before the chest is opened, the blood flowing from an incision into the right side of the heart will be found strongly saccharine. When the chest was instantly opened by Dr. Pavy after the pithing was effected, and a ligature placed around the base of the heart, the contents of the right ventricle were found to be free from sugar.

Having discovered that the sugar-forming substance of the liver was not transformed into sugar, whilst in contact with a ferment, when an alkali was present, Dr. Pavy immediately after death injected a strong solution of potash into the liver of a dog. The presence of sugar was in vain sought for, either in the liver or the contents of the circulatory system. Again, injecting only half of a liver, in the same manner, this half contained no sugar, while the other gave the ordinary amount. When the organ was allowed to remain a few moments after death before the injection was practiced, so as to give time for the post-mortem transformation of liver material into sugar to take place, the presence of sugar was as easily shown as if the potash had not been made use of.

Knowing how much changes partaking of the character of fermentation are promoted by an elevated temperature and checked by cold, the influence of a freezing mixture upon a piece of liver removed at once after death was essayed. When an animal was suddenly killed by the destruction of the *medulla oblongata*, and the abdomen instantly opened and a piece of liver cut off and plunged into a mixture of ice and salt—mixed half an hour before, to have it liquid and of a much lower temperature—the post-mortem transformation of the liver material was prevented and an absence of sugar was observed. Bernard had noticed

that if the spinal cord be divided above the origin of the brachial nerves, "a peculiar modification was produced in the liver, a sort of perversion of secretion, or rather a veritable arrest in the series of transformations that take place in the liver in order to change into sugar the albuminoid matters of the blood." (Leçons de Physiologie Expèrimentale, etc., Cours du Semestre d'hiver, 1854–1855, page 374.) In the experiment he relates, the animal was killed the day after the division of the cord; a decoction of a part of the liver was made at the very moment of the autopsy, and not the slightest trace of saccharine matter was detected; a portion of the same liver left to itself was taken twenty-four hours afterwards, and it contained an enormous quantity. After making an hypothesis to explain this, he makes a second one, which, he adds, is more probable than the first. "This new hypothesis would consist in saying that the transformations that take place in the liver require, not only nervous influence, but also a sufficient temperature, and that the temperature of the animal (a rabbit) before you being no more than 24° (75° Fah.) at the time it was sacrificed, it is not sufficient for the accomplishment of the phenomena in question. You know, in fact, that certain chemical actions cannot take place without a certain quantity of heat, and that in animals whose temperature varies, they cease when the cooling has reached a certain point, although the nervous excitability should then appear more intense, as takes place precisely in the rabbits in which we have divided the spinal cord. It may be the same for the glycogenic function; the temperature to which the animal has fallen may be no longer sufficient for determining the series of transformations that finally give sugar; and what proves that heat favors this formation is this, that after having removed the liver thus modified, if you expose it to a high enough temperature, the sugar is soon seen to appear in the tissue. *It would probably be the same if, without extracting the hepatic organ, the animal were placed in a situation where its normal temperature, which is* 37° *or* 40° (99° or 106° Fah.) *would be maintained without allowing the cooling to take place.* If, on the contrary, the liver should be exposed to a low temperature, sugar would form therein but very slowly." (Pp. 378–9.) We have extracted the whole of the passage, for it is interesting to notice how M. Bernard's

hypothesis coincides with Dr. Pavy's manner of interpreting these phenomena. The experiment suggested in the part we have italicised was performed by Dr. Pavy, and a rabbit, after having its spinal cord divided as above said, was placed over an engine boiler where the thermometer stood at 88°. At the end of three hours the animal was killed, and the indication of sugar in the liver was as strong as if the specimen had been derived from an animal which had been suddenly sacrificed without having been submitted to any previous operation. Dr. Pavy found, also, that when the temperature of the animal was reduced by other means than by a division of the spinal cord, the same condition of the liver was met with. By oiling the coats of animals so as to deprive them of their non-conducting investment, and exposing them to cold, their temperature rapidly falls; if killed when the temperature had descended to about 70° or 80°, the liver was found free from sugar immediately subsequently to death, and became strongly saccharine afterwards, precisely as in the case of section of the cord.

With frogs, Dr. Pavy found in his experiments that when they were killed at an elevated temperature the post-mortem transformation of liver material into sugar takes place—as in warm-blooded animals—so rapidly, that sugar is found under the ordinary process of examination. When, on the other hand, the animal possesses a low degree of temperature at the time of death, the post-mortem formation of sugar is so retarded, that an examination of the liver is easily effected before a change has taken place to any considerable extent. M. Bernard, it may be interesting to state, obtained precisely the same results, and communicated them to the French Academy in March last; he attributes, however, these results to the "*ralentissement de la circulation qui est lié à l'abaissement de la température!*"

From a consideration of the facts brought forward by Dr. Pavy, we think it must be concluded with him that though the liver and the right ventricular blood are strongly saccharine *after death*, yet that this is not their condition *during life*. It would appear that there is formed in the liver a substance which is convertible with extreme facility into sugar, by a process allied to fermentation; that this substance seems to have the power to resist this

transformation while located in the tissue of the living and healthy liver, but with the destruction of life this power of resistance is at an end, and the organ then becomes speedily charged with the saccharine principle. Until other facts are made known, we feel inclined to accept the conclusions of Dr. Pavy, which not only appear to be true themselves, but also enable us to explain the difference in the results obtained by other experimenters. It may be added, that, in accordance with them, diabetes must be considered as dependant upon some defect in the functional performance of the processes of the liver, owing to which the substance naturally produced by it is incapable of resisting transformation into sugar, as it does under normal circumstances.

The second part of this communication is on the Influence of Diet on the Liver; and before giving an abstract of its contents, as we have just done of the first part, we will, on account of the interest now attached to the subject, recall, in as short a space as we can, the present state of the science on this point, at least so far as the influence of diet upon the quantity of sugar-transformable material in the liver, is concerned. M. Bernard, in a memoir read before the Academy of Sciences, March 23, 1857. maintains that "the liver of dogs nourished exclusively with meats possesses the property, special and peculiar to that one organ, of creating a glycogenous matter analogous to vegetable starch, and having the power, as it, of becoming afterwards transformed into sugar, in passing by an intermediary state to that of dextrine. In herbivorous animals the glycogenous matter is likewise produced in the liver, but he does not doubt that some is also formed at the expense of amylaceous matters." The most important paper that has appeared in contradiction to these doctrines of M. Bernard is that of M. Sanson, entitled, "On the Origin of Sugar in the Animal Economy," published in Brown-Séquard's Journal de Physiologie, for April 1858. According to M. Sanson, it is established, by the results of his experiments, that the source of sugar in the animal economy is always exterior, and resides exclusively in the alimentation, whether this be of a vegetable or an animal nature. "In the normal conditions of their existence," says M. Sanson, "herbivorous animals find the amolaceous principle in the vegetables by which they are

nourished, and where it is, as we know, elaborated in great abundance. They transpose it into dextrine, a part of which undergoes, in their own economy, the whole series of metamorphoses by which this principle must pass in order to serve for the support of life, and be destroyed and eliminated; the rest of this same dextrine is accumulated in their tissues, or, rather, is incessantly present, by the fact of the constant access of the blood that contains it in excess relatively to their physiological necessities. The carnivorous, in their turn, to the alimentation of which these are destined in the far-seeing economy of nature, find in this excess of dextrine the primitive element of the sugar necessary for the accomplishment of their function, and consequently are not forced to elaborate it." (Loc. cit., pp. 272, 274.)

This paper of M. Sanson was the occasion of a report to the Academy of Medicine, in which M. Poggiale, the reporter, says:

"In the present state of the science nothing seems better demonstrated than this, that the glycogenous matter met with so abundantly in the liver of carnivorous animals is not furnished by their food. Whatever be the alimentation, whatever be the aliments on which we operate, a great quantity of sugar is constantly found in the liver, while its presence in other parts of the organism is accidental, and is only due to certain physiological conditions that can most generally be modified. The glycogenous matter is only found in the other organs of herbivorous animals when these animals are nourished with aliments rich in amylaceous substances."

The views of M. Sanson are not sustained by the experiments of Dr. Pavy, which would show that the so-called glycogene belongs particularly to the liver, and is secreted by it from saccharine, amylaceous, and other principles contained in the blood circulating through its capillaries. The facts which he has brought forward, showing that sugar does not belong to the healthy liver during *life*, alter, however, the position of this subject, so as to render unnecessary the discussion of whether the production of sugar results from a process of secretion or of a simple chemical transformation. Whatever in future may be definitely shown to be the particular purpose of glycogene, or hepatine, as Dr. Pavy called it with more propriety, as it is not

sugar producing in the economy of life, it certainly does not appear to be formed, as has been thought, for the object of transformation into sugar.

In experimenting upon the influence of diet upon the liver, Dr. Pavy found that the size of this organ is influenced to a most astonishing extent by the kind of food that is given; and that the alteration it thus undergoes is chiefly, if not entirely, due to the relative amount of hepatine present. The processes by which he obtained his results are carefully detailed, particularly the manner of collecting the hepatine, and they appear to us to be deserving of all confidence. The exact composition of hepatine he states that he is not acquainted with; and as it is a matter of some interest we will give it here, on the authority of M. Pelouse, as carbon 39.8, hydrogen 6.1, oxygen 54.1; corresponding to the formula C_{12} H_{12} O_{12}.

When dogs were kept exclusively on a diet of animal food, the relative weight of the liver to the animal was 1–30; and the relative amount of hepatine in the liver was 6.97 per cent. When they were kept upon a diet of vegetable food the relative weight of the liver was 1–15; and the quantity of hepatine was 17.23 per cent. When they were fed upon animal food with an admixture of sugar, the relative weight of the liver to the body was 1–16½; and the amount of hepatine yielded was 14.5 per cent. Some experiments which Dr. Pavy records, that he has made upon rabbits, are strikingly corroborative of the deductions he has drawn from the observations on the dog.

If this hepatine is not naturally formed for the purpose of transformation into sugar, the question as to what naturally becomes of it is most interesting. Dr. Pavy believes that in its solution attention will have to be specially directed to the bile and to the production of fat, a material which is undoubtedly largely formed, particularly under certain kinds of vegetable food, in the animal system.—*American Jour. of Med. Sci.*

EDITORIALS AND MISCELLANEOUS MATTER.

UNDERBIDDING.

It is fair to suppose that a man is willing to pay for an article the price which he demands for it. In this way we arrive at his idea of its value, both relatively and intrinsically. Thus, if a given number of persons present a certain commodity for sale, and one of them demands only half price for it, the conclusion is inevitable, that, in his judgment, it is worth less than the sum asked by the others, and is inferior in some particular to theirs. This truth finds a daily illustration in the business affairs of men, with whom it is an axiom, that the sale of an article at less than its market value is *primâ facie* evidence either of its unsoundness, or of some corruption on the part of its vender.

This principle has a far wider application than is generally supposed. So far from being limited to the commercial world, it extends to every transaction of life in which a *quid pro quo* is demanded for articles delivered or services rendered by one party to another. It has a most special and significant bearing upon the medical Profession, some of whose members, we apprehend, will appreciate their connexion with it, without further explanation from us. But, as there may be others who are too blind or perverse to make a proper application of it, we will venture to do so for them, even at the risk of exciting their indignation, or appearing censorious. Among Physicians generally, there is a certain recognised tariff of charges, varying in certain particulars in different localities, but essentially the same in principle everywhere. In some instances this standard is agreed upon by the solemn compact of the interested parties; then again it is only conventional; but as a general thing, there is very little difference as to what constitutes a proper remuneration for professional services. When then, the Physicians of any city, town,

or neighbourhood have agreed upon a certain rate of compensation, either by formally subscribing to it, or tacitly acting upon it, that becomes *ipso facto*, the legitimate value of their services and as such bears the same relation to them, as the *market value* to all articles of merchandise and commerce. The man who ignores this standard, and demands a smaller compensation for medical attention, not only does a wrong to the Profession, but, actually furnishes the most conclusive evidence of the fact, that in his own estimation, his services are really worth less than those of his brethren. There is a great deal of strong sense among the masses, and though the idea of paying smaller bills may please for the moment, the public is not slow to apply the rule we have enunciated, and to graduate its appreciation of the Physician's attainments by the valuation he himself has imposed. It is for this reason that permanent success is never the reward of this pernicious system. Hence it is that such a man makes no more than a cross-roads reputation, and is constrained eventually to surrender his practice to every rival with whom he is brought in contact. This explains why only insignificant cases fall into his hands, whilst more difficult ones are entrusted to the care of others. This gives the rationale of that process by which he descends from the mansion to the kitchen, and finally from that, to make room for some more respectable practitioner. And it is on this account that the whole country is infested with swarms of vagrant practitioners, who, having outlived their "first impression," are driven by dire necessity to roam about in search of some pest locality where professional huckstering may be repeated—at least with temporary success.

The community in which a man lives, is sure to place the same estimate upon him as he puts upon himself, unless there be associated with an exalted opinion of his own parts and attainments, an offensive vanity which brings him in collision with those

around him in such a manner, as to interfere with their self-esteem. When then, it becomes evident that a Physician regards himself as inferior to his rivals, there is no power upon earth which can prevent his depreciation in the estimation of the public, or render his professional career anything else than an ultimate failure, even though inferior men be his competitors. That this is a correct view of the subject, we are assured both from our limited observation, and the testimony of others more experienced than ourselves; and hence the confidence with which we have made the announcement. Self-interest thus demands at the hands of every member of the Profession, the most prudent circumspection in this matter, for a fatal step in the outset of professional life, is of all things, the most difficult to retrieve, and the man who once loses ground in public estimation, has more than a Herculean task to recover it entirely.

Let every man, in the incipiency of his career, realize that the success which results from underbidding is necessary ephemeral, that the practice thus secured it usually lost in the end, that such Physicians are employed as a matter of speculation alone, and not from any respect for their character, or confidence in their attainments. Let them not be deceived by the temporary advantage thus secured, for they will learn by bitter experience that in pandering to the cupidity of the public, they have destroyed the prèstige of their position, and sacrificed all that an honorable Physician holds dearest and most sacred! Let them build up their hopes of future usefulness and permanent success upon some other foundation than the transient patronage obtained by professional venality, for they will eventually discover that, instead of establishing a reputation as skilful practitioners of medicine, they have only convinced the world of their avarice and meanness!

Professional honor is like female chastity, the slightest blot is

an indeliable stain, a single lapse an irretrievable error, a breath of suspicion a sentence of condemnation; and he, who does not live above reproach, who fails to avoid even the appearance of wrong doing, and who commits himself to aught, which in the estimation of any right-minded man is even of doubtful propriety, does himself the rankest injustice, and his vocation an irremediable injury.

MEETING OF THE STATE SOCIETY.

It is a source of regret that we can not give give a full account of the proceedings of this body at its recent meeting in Statesville, as they are not only particularly interesting in themselves, but of vast importance to the whole profession. A larger number of delegates was present than on any previous occasion, whilst the enthusiasm manifested was entirely without a parallel in the medical history of North Carolina—a most telling commentary on the infatuation of those who persist in their senseless denunciations of our noble State. But that which gave especial character to the meeting was the acceptance of the "Act creating a Board of Medical Examiners," and the election of suitable persons to constitute that Board, according to the provisions of the Bill. After considerable discussion on the merits of the Bill, and the proper method of appointing Examiners, the Society took a sensible and practical view of the whole matter, and proceeded with great unanimity to discharge the important duty of electing a Board.

The following are the gentlemen selected by the Society for this responsible position :

Dr.	James H. Dickson,	Wilmington.
"	C. C. Johnson,	Raleigh.
"	W. H. McKee,	Raleigh.
"	C. Happoldt,	Morganton.
"	O. F. Manson,	Granville.
"	J. Graham Tull,	Newberne.
"	C. Winslow,	Hertford.

This is a most important move on the part of the Society. It is a triumph over which every friend of his calling must rejoice —a victory for truth and science, the prouder, because won against the most fearful and overwhelming odds—a rentable progression, such as seemed impossible in these degenerate days of quackery and politics. If the Journal has done ought towards the accomplishment of this good work—and such devotion as ours could not prove entirely barren of fruits—we are more than compensated for the labor so lavishly bestowed upon it, and feel prouder of our bantling than if crowned with the richest trophies ever won on polemic field.

The character of those composing the Board is a sufficient guarantee that their work will be faithfully and fearlessly done, so that we are justified in believing that a brighter and a better day is dawning upon the Profession in North Carolina—a day in which quackery will "hide its diminished head," and retire in disgust to some darker and more genial locality wherein its base practices may be pursued without the fear of molestation. This measure will of course be unpopular with the schools, for it conflicts materially with their interests in several important particulars, and some of them will doubtless endeavor to inspire their graduates with such a contempt for it as will induce them to attempt its humiliation. A few salutary examples will, however, obviate all difficulty on this score; for the disgrace of a rejection will not only fall upon the head of the unlucky victim himself, but will recoil with terrible effect upon the institution which has granted a Diploma to an ignoramus or a rowdy. It is the duty of the Profession to throw the whole of its influence in favor of this Board, and to sustain it manfully in all emergencies, so long as its members continue worthy of respect and confidence. In this way it will be made a most potent instrument of good—securing incalculable benefits to the Profession, covering the whole State with honor, and overwhelming quackery with shame and

confusion .But if it be left to stand alone—if the interests of the the Profession and the Board are brought in collision—if there be the slightest seperation of sentiment between the two—if the one becomes indifferent and the other independent—then is its usefulness destroyed, its days are numbered, and fall it will, involving in its destruction the utter ruin of all that we have so long and faithfully struggled for. The honor of the profession, the existence of our State Society, and the highest interests of every physician in North Carolina, are identified with the operation of the Board, and dependent upon its success. The most important issues are thus staked upon this measure,—issues which involve the triumph or the humiliation of legitimate medicine,—which affect the well-being of society as well as the advancement of our Profession, and in which posterity has as deep an interest as ourselves. It becomes every man, then, with whom such considerations as these have their proper weight, to sacrifice his individual prejudices and opinions,—to abandon the position of antagonism in which his dissatisfaction with particular provisions has placed him,—to disregard the imagined imperfections of the Law,—and, for the sake of the Profession, to stand manfully up for the Board at all hazards, and in every emergency. Let the plan be thoroughly tried, before its sentence is pronounced. Let the practical operation of the law be the standard by which to determine its merits. Let the good or evil actually done the Profession by this Board give character to our feelings and conduct respecting it. This is the sensible, practical, and equitable view of the subject, and, as such, we are convinced it will commend itself to Profession generally.

Of the further proceedings of the Society, we will speak more fully at another time,—suffice it for the present to say that the Journal was fully endorsed, and that ample means were provided for its future support.

In regard to the kindness, courtesy, and entertainment extended to the members of the Convention, it is only necessary to refer to the locality in which they assembled, and to the people who surrounded them,—for hospitality is indigenous to the soil of Carolina, and is cultivated by none more successfully than the hardy an generous sons of the West.

A VOTE OF THANKS.

The State Society thought proper to express its appreciation of the manner in which the Journal has been conducted, by passing unanimously a vote of thanks to its editor.

For this distinguished compliment, we are truly thankful, for to one who has labored faithfully to do his whole duty, such evidence of kindness and appreciation are particularly grateful.

Inspired by a proper sense of the personal partiality which prompted this testimonial, as well as by the honor itself, it will be our constant labor to render the Journal more creditable to the Society, and worthier the patronage of the Profession.

OUR PATRONS.

As this is the last number of the present series, we wish to say a word to our subscribers. A large majority have promptly paid their dues, and to all of these, it will afford us much pleasure to send the Journal for the future. There are, however, some who have failed either to return the Journal as requested, or to forward the subscription price—and to them we have to make a few remarks. You are in debt to the amount of three dollars to the Journal,—this is either accidental or intentional, and you can very easily show what your motives and intentions are by sending on the sum immediatly. Just so soon as the money has been received, we will esteem you one of the cleverest fellows in the world, and shall be happy to continue our acquaintance. If you do not intend to pay at all, and think to gratify the natural cupi

dity and meanness of your nature, by defrauding the Journal,—we are determined to thwart you in this particular at least, and to force you into a settlement, however repugnant to your feelings. You will therefore please consider this a receipt in full for all claims against you for the past, present, and future,—the *value received* being the pleasure of erasing your name from our list, and of ascertaining exactly who and what you are.

We will make this a receipt more specific if required.

DELAY.

The delay in the issue of the present number, is occasioned by the fact, that it was kept open, in order to ascertain what was done at the meeting of the State Society,—as we knew that our subscribers generally felt the deepest interest in the proceedings, and were anxious to ascertain the result of its deliberations.

We had hoped to give a more minute account, but find it impossible to do so in this issue. When the transactions are published, we will make them generally public by transferring them to our pages.

THE WARREN PRIZE.

It will be seen from the advertisement of the Medical College of Virginia, that Dr. Thomas D. Warren, of Edenton, again offers a prize of $100 for the best essay from the members of the next graduating class of that Institution. Such liberality deserves to be chronicled, that the generosity of Dr. Warren may be known and appreciated by the whole Profession.

THE VIRGINIA MEDICAL AND SURGICAL JOURNAL.

We exceedingly regret that the May number of this Journal reached us too late for a proper notice of its strictures on the Medical Bill. We willingly take up the glove thus thrown down, and shall enter the lists in August, prepared to do our *devoir*, as a loyal Carolinian should.

JOHN HUNTER.

The remains of the great John Hunter were discovered in the

vaults of the Church of St. Martin's-in-the-Fields after a *long search*, by M. F. T. Buckland, Surgeon in the Life Guards. The coffin was well preserved, and the inscription clear and distinct. It runs thus: John Hunter Esq., Died Oct. 16, 1793, aged 64. The remains have been removed from their obscure resting place, and reinterred in Westminster, with great ceremony, and in the presence of a large number of professional men. It is now proposed to erect a statue to this illustrious man, and a subscription for that purpose has been opened. "He who deserved a temple" has at length found an appropriate tomb within the hallowed precincts of Westminster Abbey, among the honored dead of England.

DR. GEORGE B. WOOD.

We regret to state, that this distinguished gentleman has already resigned his connection with the Philadelphia Hospital, and has announced his determination to give up his Professorship at the close of the approached session. This is a national calamity.

CONVENTION OF TEACHERS.

About one-half the medical schools of the country were represented at Louisville, in the Convention which assembled there one day, in advance of the meeting of the National Medical Association. The largest and most prosperous schools, as a general thing, declined to send delegates, and took no part in the movement. Had the teachers who did pretend to desire some reform in the educational system of the country, shown the slightest disposition to do anything towards affecting it, they would have astonished the whole Profession. Nothing was expected, and nobody was disappointed. If the respective States would follow the lead of North Carolina, and establish "Inde pendent Boards of Examiners," the Profession at large would be in a position to dictate to the schools, instead of being constrained to employ the language of supplication and entreaty. Just so long as men are willing to entrench themselves behind their "chartered rights," the *laity*, as we are contemptuously styled, need look for nothing of practical value from a "Convention of Teachers."

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